

A
CONTINUATION
OF NEW
EXPERIMENTS
PHYSICO-MECHANICAL

Touching the
SPRING and WEIGHT of the AIR,
And their EFFECTS.

The Second Part :

WHEREIN
Are contained divers EXPERIMENTS made
both in *compressed* and also in *fartitious* AIR,
about FIRE, ANIMALS, &c.

Together with
A DESCRIPTION of the ENGINES
wherein they were made.

By the Honourable ROBERT BOYLE,
Fellow of the Royal Society.

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THE P R E F A C E

T O

The LATINE Edition.

After I had first published my Physico-mechanical Experiments to the Curious World, and, some years after, the Continuation of them, (together with a full Description of the Engines, and lesser Vessels, which I used in the making of them) I thought it a very venial thing in me, if, superseding any farther labour upon such Subjects, I left that Argument to be studied, and, if they had pleased, cultivated by others. And therefore I was content to annex onely some Experiments, occasionally made, concerning Respiration, concerning the scarce credible Rarefaction of the Air; and lastly, concerning the Preservation of some Bodies, whilest they are defended from the contact of the Air, in regard those Tracts were of kin

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to

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to other Arguments, which I had occasion to handle at several times. But in seven or eight years space, hearing of very few Experiments made, either in the Engine I used, or in any other made after the model thereof, I began to reassume some Thoughts, concerning the farther use thereof my self: At which time it happened very opportunely, That a certain Tract written in French, small in bulk, but very ingenious, containing sundry Experiments concerning the Preservation of Fruits, and some other Tracts of a different nature, was brought unto me by Monsieur Papin, who had joined his Pains with the eminent Monsieur Christian Hugenius, in making the said Experiments; And, upon farther discourse with him, finding that he came out of France into England but a little before, in hopes to obtain some Place here, which might be fit for his Genius, and, whilest he was in that expectancie, that he was willing to bestow his Pains about Experimental Philosophy, upon which, I had an Inclination, at my cost, to gratifie his Curiosity, whilest I also indulged my own. And, seeing he had a Pneumatick-pump of his own, made by himself, to the Use of which he was more accustomed, though it differed from the structure of my Pump, I gave him the freedom to use his own, because he best knew how to ply it alone, and (if any disorder should happen, from the luxati-

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on of its Parts, or any other casualtie) how to repair it more easily. Though, in his absence, I chose rather to use my own Pump, both because my Domesticks were better acquainted with it, and also because it was not subject to so many and frequent Inconveniencies, by reason of its more solid structure.

But, seeing several sorts of Experiments, long since made on divers Bodies, had left me little to doe about the same Subjects; there were only two things, which I chiefly designed to prosecute. One of which contained those Experiments, which, when I first published my Physico-mechanical Experiments, I had wished in general had been made, not in rarefied or expanded, but in condensed or rather compressed Air. The other was to be versant about those Trials which were not to be made, as the former, with natural Air, either in its wonted state, or any way rarefied, but with factitious Air, (that I may so speak,) such as, in my former Writings, I had mentioned to be producible by the help of Fermentations or Corrosions; The divers waies of producing or extricating that factitious Air, and the waies of Trying it, when it was produced, having been some years ago presented to the Royal Society, I was invited, by that Learned Assembly, to prosecute farther those Disquisitions. Now, although those were the chief kinds of Experiments which I applied my mind unto, yet it will

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appear by the following Sheets, that I did not confine my self to them alone.

But, before I could make any considerable progress in this Work, it pleased the most Just and Wise God, the Supreme Arbiter and Ruler of all things, to afflict me with the Stone (the Pains whereof do as yet now and then trouble me) so that I was enforced to take another course of proceeding. For, to ease my self, it was judged meet, that Monsieur Papin should set down in Writing all the Experiments and the Phænomena arising therefrom, as if they had been made and observed by his own Skill; and moreover, the Calculation of the Degrees of the Rarefaction and Condensation of the Air, included in our Mercurial Gage, was intrusted to his Care. But I my self was alwaies present at the making of the chief Experiments, and also at some of those of an inferiour sort, to observe whether all things were done according to my mind. But, as for those Experiments which required a longer time in makeing, such as those about the Conservation of Bodies, he did from time to time, with great diligence, acquaint me with those Alterations, which happened in them, in my absence; and he also brought the Glass-instruments to me, and declared to me the Effects of the Experiments, when they were finished, that so I might take into consideration the Changes made in the

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the Materials, when taken out of the Vessels. Yet, I confess, I was purposely somewhat more incurious and remiss about those Experiments which were made concerning the Preservation of Fruits, and of Flesh in Liquors, which was made chiefly by the help of Compression; and also about the Coction of Meat. For, as some of these later Experiments were propounded for Tryal by Monsieur Papin, for a particular End of his own, somewhat different from my Design in the other Experiments; so I was very willing, that he should use his own method about them; not doubting but he would use his greatest Industry therein, as I found, by the Event, that he had done. Yea, I did judge, that I might more safely acquiesce in his Relations, concerning the Experiments about Flesh, about Fruits, and about Boiling of Meat, because, as these were some of the last which we made, so I had cause enough to trust his Skill and Diligence used about the former Experiments; some of which, viz. those which are marked with an Asterisk, he himself propounded, as if they had been formed in his own brain, as also not a few of the Mechanical Instruments, (especially, the Double-pump, and Wind-gun) which sometimes were of necessary use to us in our Work, are to be referred to his Invention, who also made some of them, at least in part, with his own hands.

In

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In the following Tract, the Reader will not find the Reasons subjoyned, which moved me to make these Experiments, (which I usually did in my former Physico-mechanical Experiments, and in the Continuation of them) for I had neither leisure, nor a mind free from other busineses, to make such a Preface; and I did also hope, that the sagacious Reader would find out my Sense well enough, though purposely not expressed in plain words, if he did but attentively consider the nature of the things treated of, especially if calling in to his aid those short Corollaries, which he will find annexed to the several Experiments, whereby he may fish out my aim. Though, to speak the Truth, some few of those Inferences owe themselves more to my Assistant than to me.

I am well assured, That very many of the following Experiments will not be thought weighty enough by many Readers, as to deserve to be printed, and indeed I my self was so far of their mind, that I had once thoughts of expunging them out of the following Collection; But at last I was more easily persuaded to afford them a place amongst the rest, because, however they may be considered apart, yet, in consort with the rest, they may be, at least, of moderate use.

*I was not very solicitous about the style, because, being infirm in point of Health, and besides, surrounded with many busineses, I was enforced to leave
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the choice of words to Monsieur Papin; my chief Care being to have the whole Worke diligently read over to me, that so no mistake might pass by unobserved about the Experiments themselves. Besides, seeing the things here treated of are meerly Physical, and their manner of handling but Historical, there is no need of any farther Apology, to excuse the incompletness of the style: Yet this may be alledged in excuse thereof; That the Heads of things (or Memorials as they are called) being at first set down, for haste, by Monsieur Papin in his own native Tongue, scil. the French, and afterwards turned into Latine, (in which habit they now appear) do labour with that inconvenience which doth usually attend all Translations, especially where the Interpreter must have a greater care of the Propriety of words, than of the Elegancy of them.

Moreover, he that shall attentively consider the following Experiments, will not wonder, that they are delivered in a less accurate method. For we accounted it sufficient for our purpose, to reduce those Experiments, which did differ and had least affinity amongst themselves, into some certain Heads, to which they seemed most commodiously to be referrable: And, besides, considering the nature of the Experiments themselves, I hope the Reader will easily grant, that at least many of them ought to have been set down in

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the way of a Diary, yet distinguished and, as it were, intercalated by frequent intervals, because the Examination of some of them was protracted for many days, the nature of the Experiments themselves, and also the design of the Experimentators requiring such Chasms: Add hereto, That I was more willing to set down divers things, with their minute circumstances, because I was of opinion, that probably many of these Experiments would be never either re-examined by others, or re-iterated by my self. For though they may be easily read, when set down with Pen and Ink in Paper-sheets, yet, he that shall really go about to repeat them, will find it no easie task.

For there are so many, and such sundry sorts of Instruments, both of a greater and lesser size, which are necessarily required for use herein, some of them to be made on purpose for the present occasion, respect also being had to the time and assiduity, requisite and necessary for making the Experiments and Observations, in cases wherein so subtile and elastick a Body as the Air is, must be violently reduced into a preternatural state, and must be long kept in that disposition, that, as it is a very difficult thing to prevent those Inconveniences which do attend so unusual Experiments, so it is far more difficult, to apply Remedies to those Inconveniences, after they have once happened. For these, and other Reasons, so much time
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is to be spent, that I am almost ashamed to tell how much thereof was impended on these Trials which are contained in the present Book, though but small, to which this Proeme is prefixed.

Nevertheless, though all these things are alledged in excuse, yet the deficiency of this Collection is so well known to me (there being little to be found therein which may commend Books) that I would invite very few Philosophers to the reading of so incult and unpolite a Rhapsodie, especially from the beginning to the end. For though it may probably happen, that some Experiments, contained herein, may not be disallowed by the Curious, yet they may have leave from me, to esteem this whole Tract but as a loose Heap (or rather Chaos) of Particulars belonging to the Air, especially, as constituted in its preternatural state, and to the operations of it upon some bodies, as clothed with such and such circumstances; so that it is free for them to cull out onely those Experiments which please their Curiosity, or any other of their Concerns best, without being obliged to read over the whole Book, no more than a Lexicon, which we use not to consult, but now and then, for the sake of a word. In short, 'Tis not probable, That a Book so impolite, as this is, will be either wholly read over, or can conciliate any favour from the reading, unless with those Readers to whom a Book comes sufficiently

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commended onely upon this accompt, That it contains things New and also True. For if those two Privileges are enough to obtain Favour, then there is no cause, that the following Tract should wholly despair of the Reader's benevolence, especially since some Trials contained therein do treat of the Properties and Operations of the Air; I say, of the Air, which, notwithstanding the laudable Endeavours of some ingenious modern Writers in the Explication thereof, yet is a Body which, I fear at present, we have greater use and necessity of than knowledge.

An

An ADVERTISEMENT of
THE
PUBLISHER to the READER,
Before the Latine Edition.

Several Tracts, made by our Author, printed at *Geneva*, and bound up in one Volume, were not long since transported into *England*: In which matter, though the Author himself doth not complain (which yet he might lawfully doe) of the immoderate Liberty of some men, who have presumed, unknown to him, to bind up so many of his Writings together, and to publish them. Not to mention the Print, as being but bad, (or at least not accurate) yet there are two things in that Edition, which; in our Author's behalf, cannot be concealed without just reprehension, for they may empair his Credit much, especially with those to whom his Writings are no otherwise known than by that Collection.

For, First, There is no Signification made therein, That any of Mr. *Boyle's* Tracts were ever written in any other Language than that wherewith they are there clothed, viz. The *Latine*, whence it may probably come to pass, That all the Faults and Defects of Style, which are wont to blemish Translations, especially such as are literally made, may, by Readers, who are not otherwise enformed, be imputed to the Author himself, who, for Reasons often rendred by him, was induced to write all his Works in the *English* Tongue: The Versions of some of them into *Latine* being not so much as seen by him, till, being come from the Press, they were put into his hands.

Secondly,

An Advertisement of the

Secondly, The several Tracts making up that Collection, are all dated in one and the same year, *viz.* 1677. as if they had been all, both writ, and also published, by our Author at once, whereas indeed some of them were made publick 8 or 10 years, some 11 or 12, others 17 or 18 years before ever this Collection saw the Light: Hence an Injury, greater than the former, may be offered to our Author; for those Readers, to whom neither Himself, nor his Lucubrations are known, but from that Volume, may be easily persuaded to believe, that those Experiments, if perhaps they meet with the same which are comprehended in these Books, and are also found in other mens Works printed before 1677, were transferred by our Author out of their Tracts into his own; than which nothing can be imagined or spoken at a greater distance from Truth: For, indeed, if, applying my self for three whole years to manage the Experiments of so Great a Person, and thereby having frequent opportunity to converse with him, I sometimes casually light upon something new, yet who sees not, that Thanks is to be returned to him alone, who afforded me both the Occasion of meditation, and also Leisure to operate; yet such is the Humanity of this Noble Person, that he mentions my Name in the Preface to this Book; as if some things therein were mine: Who then can justly say, that he hath excerpted any thing from other Authors, who gives his own freely unto others? But, to make the matter more clear, and also, to satisfy some Ingenious Persons who have earnestly desired a Catalogue of all Mr. Boyle's Works, I will here subjoin it, and also affix to each Tract the time of its Publication; for by this means any Enquirer will be able to perceive, that what was written by our Author for New, hath really been published before the Writings of all the rest. And besides, the Faults of many will be detected; for though some Writers have with Ingenuity enough cited the Name of our Author in their Works, yet more have done otherwise, transferring not a few
of

Publisher to the Reader, &c.

of his Experiments, together with the Ratiocinations explaining them, after the manner of Plagiaries into their Books, making no mention of his Name at all.

But here I must advertise the Reader of these two things:

1. That those Books, marked with an Asterisk, were long since turned into *Latine*; yea, some of them but a little while after their Editions in *English*; yet without any Additaments in their Versions.

2. The other, which might have been set in the first place, is, to hint the Reason, why this present Tract bears the Title of *Continuation, &c. Part the Second*. For you must know, that after the first *New Physico-mechanical Experiments* of our Author were published to the World, some years after, a large Continuation of them in *Quarto* was likewise printed, which was also translated into the *Latine* Tongue, but, by the Death of the person to whom the Charge of publishing it was committed, and other Accidents happening thereupon, that Version could not yet be found; and if no hope do appear of recovering it again, (which we do not wholly despair of) then probably a second Translation may be undertaken, for the sake of the Curious.

A

A
C A T A L O G U E
Of all the
P H I L O S O P H I C A L W O R K S
Published by our AUTHOR.

* **N**EW *Physico-mechanical Experiments concerning the Weight and Spring of the Air; published in English, Anno Dom. 1660.*

* *A Continuation of them, Part I. 1669.*

* *The Defence of the New Experiments, &c. against Franciscus Linus.*

The Examen of the Physical Dialogues of Thomas Hobs, concerning the Air. These two were published, A.D. 1661.

* *The Sceptical Chymist, 1661.*

* *Physiological Essays, together with the History of Fluidity and Firmness, and some other Tracts, Printed 1662.*

* *The Experimental History of Colours begun, A. 1663.*

Concerning the usefulness of Experimental Philosophy; the first Tome: A. 1664.

The second Tome was printed, 1669.

* *A Tract concerning the Origin of Forms and Qualities, 1666.*

Though this Tract was turned into *Latine* divers years before the *Genevian* Collection was published, yet was omitted therein, whence it appears, that the Publisher was not very cautious, who affirms in his Preface, That all Mr. Boyle's Works are contained in that Volume.

The

A Catalogue of the Author's Books.

The Experimental History of Cold begun, to which is subjoined a Dissertation concerning Antiperistasis, together with an Examen of Mr. Hobs's Doctrine about Cold; 1665.

* *Hydrostatical Paradoxes; 1666.*

* *The Origin of Forms and Qualities; the second Edition; to which is annexed a Dissertation concerning Subordinate Forms; 1671.*

* *Traacts concerning the Cosmical Qualities of things; Cosmical Suspensions; the Temper of the Marine Regions; the Temper of the Subterranean Regions, and of the Bottom of the Sea; 1671.*

* *An Essay concerning the Origin and Vertues of Gems; 1672.*

A Traact containing New Experiments between Flame and Air; together with an Hydrostatical Dissertation; 1672.

* *Some Essays concerning the wonderfull Subtilty and Efficacy of Effluvioms, and their determinate Nature; 1673.*

Some Traacts consisting of Observations concerning the Saltness of the Sea; with a Sceptical Dialogue concerning the Nature of Cold both positive and privative; 1674.

Traacts containing some Suspensions concerning some Occult Qualities of the Air; with an Appendix touching Celestial Magnets, &c. 1674.

An Introduction to the History of particular Qualities in the Philosophical Transactions; N. 63. p. 2057.

* *Of the Excellency of the Mechanical Hypothesis; N. 103. p. 53.*

Experiments and Observations concerning the Mechanical Production and Origin of several particular Qualities; together with some Reflexions upon the Hypothesis of Acid and Alcaly; 1675.

The Sceptical Chymist, or Chymico-physical Doubts and Paradoxes about those Experiments, whereby vulgar Spagyristes do labour to evince, that Sal, Sulphur and Mercury are the genuine Principles of things; to which, viz. in this 2d. Edition, sundry
b
Experi-

A Catalogue of the Author's Books.

*Experiments and Considerations are subjoined concerning the Produci-
bileness of Chymical Principles; 1680.*

** A Continuation of New Physico-mechanical Experiments;
the second Part; 1680.*

These are the Philosophical Works of our Author hither,
to published; what he hath wrote in Divinity belongs
not properly to this place; not to mention several Dis-
sertations of his which you may find here and there in-
terspersed among the Philosophical Transactions pub-
lished in Print.

THE

THE
TRANSLATOR
TO THE
READER.

T*Hough the First Part of the Physico-Mechanical Experiments of this Honorable Author was published by him in the English Tongue, as was also, some years after, his First Continuation of the same, yet so welcomly were they entertained by the Curious, especially in Transmarine parts, that the First Part hath been long since published in the Latin Tongue; and the First Continuation is also translated into the same Tongue, though (for reasons, in part mentioned at the end of the Publisher's Advertisement to the Reader prefixed before this Tract) not yet Printed.*

This Second Continuation of the aforesaid Experiments speaks Latin at the first hand; but that all those Three Tracts might be clothed with one habit, it was the desire of some ingenious Persons, that it might also be rendred into English; which Province hath been recommended to me by the Bookseller.

*I may without vanity affirm, that I have an advantage beyond some others, in reference to the Versions of any Tracts of
this*

The Publisher to the Reader.

this Noble Author, either out of English into Latin, or out of Latin into English, in that, by reason of the vicinity of my habitation, I have conveniencie, at fitting Seasons, to consult the Author himself, about his sense in any place which may be doubtfull to me; which I mention, not onely to declare the Candour and Condescension of so Eminent a Person, but also to account for any Mistakes I may be guilty of, which are therefore rendred the more inexcusable in me, and properly to be laid at my own door.

It is not to be doubted, but that, if the Honorable Author of these following Experiments had himself at first dress'them up in their English habit, they had appeared far more terse and polite than my inability can trim them up to, yet, besides the necessary Inconveniences which attend all Translations of Books, (especially those which treat of Nice and Curious Subjects) which I alledge for part of my Apology, I do farther relieve my self with that saying of the Orator, Si quis summa desperet, tamen est pulchrum in secundis tertiusve consistere. Quintil.

And in reference to the Elegancies of this Noble Author, in the English as well as other Languages (of which he is so great a Master) I may farther add with the same Orator, Ut transeundi spes non sit, magna tamen est dignitas consequendi. 'Tis sufficient honour for me to write after his Copy. In fine I conclude with the Poet;

Veniam pro laude peto, laudatus abunde,
Si fastiditus non tibi, Lector, ero.

(1)

THE SECOND
CONTINUATION
OF
PHYSICO-MECHANICAL
EXPERIMENTS.

ICONISME I.

The description of the Engine, with a double Tube for the exhausting of the Air.

AA

A

RE Two Pumps made of Brass.

BB Are Two Plugs hollow within, and open below.

CC Are Two holes in the upper part of the Plugs with Valves opening outwardly, that they may afford passage to the air to go out, and hinder it from coming in.

DDDD Are Iron Rods serving to move the Plugs, and annexed to them; by means of the Gnomons FF.

EE Are Two flat Iron stirrups at the top of the Rods DD, on which the Operator must stand to set a work the Engine.

GGG Is a Cord joyned to the Two Stirrups, and compassing the Pully H.

B

LL

LL Are Two Valves at the bottom of the Pumps, opening inwardly, for the admission of the Air out of the Tube MM.

MM Is a Tube reaching from both Pumps to the Plate OO, by means of the Curvature PP QQ; which Curvature ought to be of so great length, that the Tube P QQ may not hinder the exerciser of the Pumps, but that he may conveniently stand on the stirrups EE.

OO Is a Plate bored in the middle, on which the Receivers, to be evacuated, are to be put; as R for example.

Before this Engine can be fit for use, it is to be put into a frame of wood to support it, as is shewed in the second Scheme, and as much water is to be poured through the hole Q in the Plate OO into the Pumps, as is sufficient to fill the Cavities of the Plugs, and a little more; and then some body must stand on the two Iron Stirrups EE, and must alternately depress and elevate them. For by this means it will come to pass, that the Plugs, following the motion of the Stirrups, in their ascent will leave the space in the bottom of the Pumps empty, and seeing all other passage is intercluded from the Air, that Air alone which is contained in the Receiver R is conveyed into the aforesaid Pumps by the Tube QQ PP M, and opens the Valve L, which being presently shut hinders the same Air from making a regress: wherefore the Plug, afterwards descending, Compresseth that Air, whence of necessity the Valve C must be opened, and all the Air must pass out at it, *viz.* because the water in the bottom of the pumps doth exactly fill all the spaces, and doth also regurgitate through the Valve C.

Here we may observe, That this *double* Engine is upon many occasions to be preferred before a *single* one (that is moved with the Foot,) for it doth not onely produce a double effect, but performs it also much more easily; for in those Engines, which are furnished but with one Tube, whilst the

the Plug is drawn up to evacuate the Pump, the whole Pillar of the Air, incumbent on the Plug, is to be elevated by force; and again, when the Plug returns back, it is also by force to be restrained, lest it should be too swiftly impelled by the Air, and so break the bottom of the Engine; but in these double Engines, the Plyer of them is in a manner wholly free from that toyle. For in the First suction, the Plugs are easily lifted up, because the Air, immediately derived from the Receiver R into the Pumps, presseth the Plugs downwards, almost as strongly as the external Air incumbent on the opposite part; and when the quantity of the internal Air is diminished, it comes to pass that the Plug to be depressed, tends downward with so much the greater force, and so by means of the Cord GGG compassing the Pully, draws the other Plug upwards, and at the same time hinders it from too much velocity of descent. And by this means both Plugs at one and the same time will be helpfull to him that exerciseth the Pumps.

Seeing the Plugs make but a very small resistance, a man may easily judge, that the two Pumps of this Engine may be plyed with greater ease and also with more speed, than one Pump in single Engines can, so that this engine is of great use in order to those Experiments, which cannot be well made, but with velocity and speed.

ICONISME II.

The description of the Mercurial Gage.

THE First description of a Mercurial Gage, to discover the degrees both of the rarefied and condensed Air, may be seen about the beginning of the Continuation of our Physico-Mechanical Experiments; but those Gages which I used

in the following Experiments, are declared in the subsequent Scheme.

Fig. 1. The whole Gage ABCDE consists of Three Glass Tubes, all very well fastned and cemented together, yet so, that a passage is open from one to the other; The first of these Tubes AB being open at the extreme A, is of less capacity than the Tube BCD, but of greater than the Tube ED. The Tube BCD is crooked in the middle, and the Tube ED ought to be Hermetically sealed, at the extreme E, but the part BCD must first be filled with Mercury.

This Instrument thus prepared, if it be put into a Receiver, out of which the Air is afterwards to be extracted, it will come to pass, that the Air remaining in the part ED, will by its spring compress the Mercury DCB and force it to ascend into the part BA, and its selfe will be dilated in the Cavity DC. If then the proportions be duely observed between the bigness and length of the Tubes, as shall be declared hereafter, when the Air is extracted, the Mercury will almost reach to the top A, and the Air in the other Leg, being so dilated, that it cannot sustain a greater body of Mercury, will be kept included in that place.

But that this Instrument may exactly tell the quantity of the Air produced in its Receiver, the Tubes AB ED are to be distinguished by marks into several parts; And when the Torricellian Experiment is tryed, above the plain Plate LM of the Pneumatick Engine, as you may see in the *Figure*, a Receiver FGE is to be taken, being perforated in the top F, and the Tube HI is to be transmitted through the hole, that so the Receiver may be applied to the Plate; and then the Hole F being stopped, and the Gage ABCDE being put into the Receiver, the Air is to be exhausted; the Air then being dilated in the Receiver, the Mercury cannot be sustained so high in the Tube HI, but must descend by degrees; and at the same time

time the Air of the Tube ED drives the Mercury by little and little into the Tube AB. When then the Mercury in the Tube HI descends to the height of 29 Digits (I take Digits for Inches throughout all this Tract) and stays at that height, if we mark to what height the Mercury hath ascended into the Tube AB, we may know, that as often as the Mercury in our Gage shall rest at that height, the Air in the same Receiver will be able to sustain onely 29 Digits of Mercury; so that the place in the Gage, or in the Paper seemably divided, must be marked with the figure 29. And so further, every Digit of the descent of the Mercury in the Tube HI may be marked in our Mercurial Gage, and the part AB will be fit to shew all the degrees of the rarefied Air.

But now if the Air be condensed in the Receiver above its wonted pressure, and all ways of its escape be stopped, you may immediately know it by the Tube ED; for the Mercury will be impelled into it by the incumbent Air, through the open hole so much the higher, as the compression of the Air in the Receiver shall be the greater; and how great that is, and what an altitude of the Mercury it can sustain, may easily enough be found out, if the computation be made after the manner following.

It is evident from the Experiments long since published by Mr. Boyle in his Answer to *Linus*, That the space possessed by the Air, is diminished in the same proportion, as the compressing force is encreased, and *vice versa*.

Let then (for Example) the space A be possessed by a certain quantity of Air, when (for instance) the compressing force is F; if now we encrease that force by the addition of G, which is equal to it, it will happen, that our self-same quantity of Air will be reduced to half its space, so that B the remaining space will be the half of the total space A, even as the former pressure F is the half of the total pressure F + G. So further, if we encrease the pressure more by the

B 3

addition

addition of H, so that the first pressure F is only $\frac{1}{4}$ of the total pressure F + G + H, it will come to pass, that the Air can possess only the space C which is $\frac{1}{4}$ of the total space A. And so afterwards, the remaining space will be in the same proportion to the total space, as the first pressure is to the total pressure.

The remaining space. The total space : :

The first pressure : The total pressure.

So that three of those terms or quantities being known, it will be easy to find out a fourth by the Rule of proportion. For instance, In our Gage let the Tube ED be the total space, in which the Air is compressed by the wonted pressure of the Air, which in *England* is wont to be equivalent to 30 Digits of Mercury, or thereabouts; and therefore the first pressure will be 30 Digits of Mercury. Now if that pressure be increased, and the Air be reduced into a narrower space, suppose into the space NE; if I would find out the quantity of this pressure, I measure the remaining space NE exactly, and I constitute that, suppose 6 Digits or Inches, for the first term of proportion; the second term will be the total space DE, suppose 12 Digits; the third term will be the height of 30 Digits of the Mercury, which was the first pressure; and so the fourth term or total pressure will be found to be 60 Digits of Mercury; whence I may conclude, that the pressure of the Air in the Receiver can sustain the Mercury to the height of 60 Digits: And so of the rest.

From the same principle before laid down, it will be easy to collect, what ought to be the proportion between the Largeness of the Tubes AB and ED. For that depends on the length of the Legs, which the higher they are, so much the better they can restrain and keep in the Air being but a little dilated in the sealed part. For instance, Let the length AB be of 10 Inches, which height of the Mercury is $\frac{1}{4}$ of the accustomed pressure

pressure, it will be sufficient that the Tube HB be twice as big as the Tube ED; for after the Mercury hath ascended to the top of the Tube AB, the Air included in the other Leg, expanding it self into the space, forsaken by the Mercury, will possess three times more than its former space, and so $\frac{1}{3}$ of the first pressure, which is 10 Digits, will be sufficient to curb its spring. But if the Legs were of less length, then the Mercury would be expelled by the included Air, at least in part. And therefore the bigness of the Tube AB ought to have a greater proportion to the bigness of the Tube ED, that the ascending Mercury may afford greater place to the Air to be dilated, and so, the spring of the Air being weakened, the weight of the Mercury cannot be overcome. And that would happen so, if the height of the Gage be to the height of 30 Digits, in the same proportion which the first space of the Air is in, to the total space, which the Air would possess *in vacuo*: According to the principle before laid down.

It is better that the height of the Tube be longer than shorter; because if it be shorter, the Mercury will be expelled in part, and so will not be able to shew all the degrees of rarefaction; but if it be longer, this onely will happen, that the Mercury will not reach to the top, and so the Gage will nevertheless indicate all the variations, though they be less sensible ones.

But the Tube DC ought to contain that quantity of Mercury at the least, which may be sufficient to fill the Tube AB, before any way of eruption be opened for the Air included in the Tube ED. If the capacity of it be much greater, the matter is not much; nor need we be very solicitous concerning the Figure of this Tube.

ICONISME II.

A description of the Engine to compress the Air.

Fig. 3. AA **I**S a Glafs Vessel, whose orifice is exquisitely fitted to the plain Plate BB.

BB Is a plain Plate of Brass, made to cover the Vessel AA exactly.

CC Is a small Tube of Brass, passing through the middle of the said Plate, and fastened thereunto.

E Is a little Valve, opening inwardly, to shut the small Tube C. aforesaid.

F Is the Spring depressing the Valve E.

GGG Is the Gnomon fastened to the Plate BB, made for restraining the Spring F.

II Is a square Lath, sustaining the Plate BB, and bored through in the middle to transmit the little Tube C.

LLL LLL Are two Iron Wires, which passing through the holes in the Lath II and compassing the upper part of the Iron Plate KK, do hinder the said Plate, that it cannot be much moved from the Lath.

KK Is an Iron plate with a hole in the middle formed into a Female-screw, to receive the Male-screw MM.

MM Is an Iron Screw, whose use is, straitly to conjoyn the Receiver AA with the Plate BB. And lest the Brass Vessel should be broken, it is convenient to put some wood with Leather between the Screw and the upper part of the Receiver: Also Leather is to be put upon the Plate BB both to prevent the breaking of the Glafs, and also for the more exact shutting of the Receiver.

NN Is a Pump fastened to the Tube C below the Plate BB.

OO Is the Sucker or Plug of the Pump NN.

P Is

P Is a little hole in the lower part of the Pump, by which the Air enters into it, when the Plug is brought to the lowest part thereof.

Now if we would compress the Air by the help of this Engine, we put the Bodies, about which the Experiment is to be made, into the Receiver AA; and laying it on the Plate BB, we firmly bind it thereto by the help of the Screw MM. This being done, the Sucker or Plug OO is to be drawn, till the external Air by the hole P, can fill all the upper part of the Pump; then if the Plug be drawn upwards, it will come to pass, that the Air finding no other way of egress, will open the Valve E, and enter into the Receiver AA, from whence there is no regress, because the valve E is presently depressed by the Spring F, and doth shut the hole C. And so we may iterate the compression of the Air into the Vessel AA, as often as we please, and the quantity thereof is easily known by the Mercurial Gages.

But I am wont so to fashion the Pump, that it may be fitted by a Screw to the Tube C. For so when one Receiver is full, we may take away the Pump, and use it to fill other Receivers.

Now because in these Engines, Mercurial Gages are used onely to shew the degrees of compression, there is no need of using the Gages here, which are described in the first Figure; for they are made with more difficulty, and besides, they afford but a small space to note the degrees of compression in. And therefore it is better to fold the Glass Tube, sealed at one end, in several places, as the Figure T shews, that a long Tube may be contained in a shorter Receiver: so that the Mercury being put in, through the open end, as much as will suffice to fill the length of one Digit, all the rest of the space filled with Air, will serve for the marking of the degrees of compression, much more sensibly than can be done in a shorter Tube.

C

Here

Here we must note, That when the Mercury tends downwards in such an inflected Gage, the weight thereof doth help the external pressure; but when it is impelled upwards, the same weight makes resistance: This difference must be heeded, if we have a mind to try very accurate Experiments.

ICONISME II.

How mixtures may be made in compressed Air.

Fig. 3. **L**ET the Receiver be AA, in which we have a mind to mix either liquors or powders.

Let QQ RR be two Tubes, each of them sealed at one end, and open at the other.

Let RQS be a Vessel of Brass, to be laid upon the orifice of the Tubes, as is shewed in the Figure.

The Liquors to be mixed must be poured into the Tubes QQ RR, each liquor in his own Tube, and let the Vessel inverted RQS be laid on the orifices of the Tubes, and in that posture let all be covered with the Receiver AA, let the Screw be wrung or straitened, and the Air intruded after the manner described fol. 9. And when you shall understand by the Gage TT, that the compression is arrived at that degree, which you intend, the Engine is to be inverted, and so the Liquors will flow down from the Tubes into the Vessel RQS, and be mixed there. If you desire to mix more liquors or powders, then the number of the Tubes is to be encreased accordingly.

ICONISME III.

How factitious Air may be transmitted out of one Receiver into another.

I Tried two ways (principally) to transmit Air out of one Receiver into another; but because the first of them seemed less convenient, I shall *here* only describe the Latter.

AA Is a plain Plate made of Metal, having an hole in the middle.

BB Is the Stop-cock fastened to the hole in the middle of the Plate AA, one of whose ends is formed into a Male-screw.

DC Is a Copper Funnel open below, with a broad orifice (that so it might be easily set upon the Pneumatick Engine and there stand firm) and in the upper part the orifice D is fashioned into a Female-screw, to receive the Male-screw of the Stop-cock BB.

EE Is a small Tube, open at both ends, both whose orifices are excavated into a Female-Screw, to receive the Male-screw of the Stop-cock BB. Fig. 2.

FF Is the Receiver laid on the Plate AA, and exactly fitted thereunto. Fig. 1.

Now if we would make factitious Air, we must put the matter which is to produce the air, into the Receiver FF, and placing the said Receiver on the Plate AA, by means of the Screw, we must strongly fasten it thereto, after the same manner as hath been described in our Engine for compressing the Air; and the Stop-cock BB we insert into the Female-screw D; then the orifice C, and with it the Receiver, is to be placed upon the pneumatick-Engine, and the Stop-cock B being opened, the Air is to be extracted; when the Receiver FF

is emptied of Air, the Stop-cock B is to be shut, that so all passage of external Air into the Receiver may be intercluded, and the Stop cock being taken out from the Female-screw D, the Receiver is presently to be immersed in water, so that at least the Plate AA with the Stop-cock may be covered therewith; for so it will be clear, that no Air from without can find ingress, and the Air produced out of the matter included in the Receiver, will be preserved unmixed, and the degrees of its rarefaction or compression are known after the same manner, as hath been described p. 4.

Fig. 3. Now if we would transmit that Air into another Receiver; another Receiver FF with another Plate AA, and a Stop-cock BB is to be procured and evacuated after the same manner, as was before described, then by means of the small Tube EE we joyn the Stop-cocks BB of both Receivers, as is shewn in *Fig. 3*, and all suspected places are to be stop'd with Cement or Turpentine, that no external Air may find admission; then, the Stop-cocks being opened, the Air produced in the former Receiver flows into the latter, and the Stop-cocks being again shut and plucked out from the Tube EE the Receivers may be kept apart; and if there be any matter included in the latter Receiver, we may easily view what influence the factitious Air hath upon it.

But because the Mercurial Gages described fol. 4. are spoiled if they be inverted, and the Gages, mentioned fol. 9. do presently expel their Mercury, if the Air be rarefied in their Receivers; and seeing the operation, here described, cannot be perfected, but both Receivers must be inverted, and both likewise emptied of Air; we must make Gages of another sort after the manner following. See *Fig. 4*.

AA Is a Glass Phial filled with Mercury to the Superficies DD or thereabout.

BB Is a Glass Tube very well cemented, in the orifice of the Phial.

CC Is

CC Is another Tube transmitted through the Tube BB, and reaching to the bottom of the-Glass. This Tube must be sealed above and open below; neither must it so exactly fill the Tube BB, but that passage may be opened to the external Air within the Glass AA.

Now if you put this Instrument into a Receiver, from which the Air must be afterwards extracted, it will come to pass, that both Tubes will be exhausted of Air, and when you invert the Receiver, to take in new Air, as in Fig. 3 is declared; the Mercury will flow down to the orifices of the Phial, and will be there kept below the orifice of the Tube BB; and the new Air entering, will easily fill both Tubes and Phial: Then the Receiver being erected, the Mercury will again be stagnant in the bottom of the Phial, and the orifice of the Tube CC will be found demersed in it. Then if any Air be produced, out of the bodies included in the same Receiver, it will come to pass that the Mercury will ascend into the Tube CC, and there, reducing the Air into a narrower place, will shew the degrees of compression.

Note that almost all the kinds of factitious Air in the beginning are in part destroyed, and therefore the degrees of compression cannot here be so exactly known, unless we know by Experiments, what part of the Air is wont to be destroyed.

ICONISME IV.

An Instrument by which Air may be filtrated through Water.

AA IS a Glass Receiver, whose orifice, laid upon the Plate BB, agrees exquisitely therewith. Fig. 1.

BB Is a plain Plate with an hole in the middle, to transmit the Tubes CC DD.

C 3

CC

CC DD Are two Tubes cemented to the Plate BB, one of which is no higher than the Plate, but the other reacheth almost to the Top of the Receiver.

EEEE Is a Stop-cock, to whose holes the Extremities of the Tubes CC DD are fastned.

FF is the Key of the Stop-cock unperforated, wherein onely one chink GG is excavated.

HH Is the Receiver, compassing the end of the Stop-cock, and fastned to it, serving against the ingress of the outward Air, and communicating with the Pump II.

LL Is a Glas Vessel.

M Is a hole in the top of the Receiver, whose Stopple is fastned with a Screw.

In the second Figure there is exhibited a Stop-cock, cut transversly, that the two Tubes CC DD may be the better distinguished, and their insertion into the Stop-cock be perceived.

This Instrument is *thus* to be used: We put the thing, about which the Experiment is to be made, into the Vessel; and the Receiver AA being laid on the Plate BB, we pour water into the hole M till the Receiver be half full, or thereabouts, and the Vessel LL, with the matter contained therein, do swim on the top thereof; then we stop the hole exactly, and fasten it with a screw, in the same manner as hath been described in the first Scheme. These things being thus prepared, the Key is to be set in that posture that the chink GG may communicate with the Tube CC; then the Plug being brought to the lowest part of the Pump, the Air of the Receiver AA, entring through the upper Orifice of the Tube CC, will flow down through the chink GG into the Receiver HH, and into the Pump. Then the Key being inverted, so that the chink GG doe answer to the insertion of the Tube DD, the Plug is to be impelled upward, and then the Air will be expelled from thence, and, finding no other passage, will be driven through the chink GG, into the Tube DD; and from thence will emerge to the upper

upper part through the water stagnant in the Receiver. Iterating this labour, we strain the Air through the Water, as often as we please; and by this means, we know whether it be clothed with any new qualities, in respect of the body included with it.

ICONISME IV.

How the same Numerical Air may be sometimes condensed, sometimes rarefied.

LET the Receiver AA be placed upon the Plate BB Fig. 3. and screwed in, as is described fol. 8.

CC Is the Stop cock, fastned to the hole in the midst of the Plate BB.

DD Is a pump joyned to the Stop-cock C with a screw.

E Is a Vessel of that bigness, that it may fluctuate in the Receiver AA without danger of inversion.

Let some Animal be put into the Vessel E, and let the Receiver AA be put upon it and screwed to it, as the Scheme shews. Then let the Pump be filled with water, and by a Screw fitted to the Stop-cock; the Stop-cock being then opened, let the Plug P be forced upwards, then the Water ascending through the Stop-cock will, in part, fill the Receiver AA, and will reduce the Air, contained therein, into a narrower space, without any addition of new Air; if then you draw the Plug downwards, the same numerical Air will be again rarefied. Thus you may both condense and rarefie the same Air as often as you please; and by this means you may find out, whether the condensation of the Air do contribute any thing to prolong the life or health of Animals, yea or no?

ICONISME II.

The description of a Wind-Gun.

AA Is a Copper Globe, hollow within.

BB Is a Tube, fastned to the Globe.

F Is a Valve opening inwardly, and shutting the Globe **BB**.

G Is the Spring depreffing the foresaid Valve.

H Is a Gnomon affixed to the Globe **AA**, and making fast the Spring **G**.

CC Is a Tube of Iron, fastned to the Tube **BB** and the Globe **AA**.

DD Is a Plug exactly fitted to the foresaid Tube.

EEE Is another Plug fitted also to the Tube **BB** with an Iron Wyre, reaching almost to the Valve **F**.

R Is the protuberance of the Tube **CC**, somewhat hollowed above to receive the end of the Iron **LL**.

LL Is a crooked Iron, moveable about the Extremity in **R**, so that it is like a leaver to lift up the Plug **EEE**.

OPO Is a crooked Iron, fastned in **M**, that the Thumb sticking in the Angle **P**, the rest of the Fingers may attract the Leaver **L**, and so force the Plug **EEE** upwards. But the Curvature is made for this use, that the one end **O** might be applied to the shoulder, if it be thought fit to aim at any mark.

TT Is a rectangle of Iron, compassing the Leaver **LL** and the Iron **OPO**, to keep the Leaver in that posture, which the present Scheme holds forth; for otherwise the Plug **EEE**, would be thrust out far away, whilest we intrude the Air into the Globe **AA**.

II Is an elliptick hole in the upper part of the Globe very well shut with a Valve, opening inwardly; whose use is to
give

give liberty of inspection, and of amending what is amiss; for the Valve may be drawn through the hole by reason of its elliptick Figure.

SS Is a metalline plate transversly placed above the hole II, and perforated to transmit the Screw V, by whose help the Valve shutting, the hole II is sustained and is applied closely to the hole.

Q Is an hole in the inferiour part of the Tube CC, by which the Air enters into the Tube, whilst the Plug D is brought to the lowest part of the Tube.

The Air is thrust into this Engine after this sort, I tread with my foot upon the crooked end of the Plug DD, that it may not be removed from the ground, and I lit the Engine upward, till the upper part of the Plug be found below the hole Q, and then the Air entring through the foresaid hole, doth wholly fill the Tube CC.

Then I forceably depresso the Engine, and so the Air, contained in the Tube CC, opens the Valve F, and is thrust into the Globe AA; whence it cannot return, because the said Valves presently stop the passage; and thus by iterated turns, we may condense the Air in the Globe, untill the force of its Spring cannot be overcome by our strength.

Now if we would discharge the Air, so condensed, the Plug DD is wholly to be drawn out, and a bullet of Lead to be put into the bottom of the Tube CC: Then by means of the Leaver LLL the Plug EEE is to be impelled upward, as we said before, and then the extremity of the Iron-wire opens the valve B, and the air breaking out therefrom, expels the Leaden Bullet through the Tube CC with great violence.

Note that before the plug DD is again put into the Tube CC for the compression of the Air, about half an ounce of water is to be poured into the said Tube. For by this means no Air at all can escape out by the Plug, and moreover, that

D

water

water exactly filling the upper part of the Tube CC, will Cause that the whole Compressed Air will be intruded within the Cavity AA, and so the condensation will be perfected much sooner, than if, at every turn, part of the compressed Air did remain below the Valve F.

This Engine is much better than any Wind-Guns hitherto mentioned in Print.

1. Because that seeing one onely Valve serves, both for the letting in, and discharging forth of the Air, it is less subject to be spoiled or impaired, than if two Valves were used for that purpose.

2. If any disorder happen in other Guns, the Engine remains useles, but here by the Elliptick hole, a man may take out the Spring and the Valve, and so mend whatsoever is amiss.

3. In other Guns the Valves being covered with Leather were put in before the Engine was on every side shut, and therefore Silver-solder could not be used in cementing the parts, but onely Lead-solder by which the Air, being much compressed could by no means be restrained; but here all things are well cemented with Silver solder, without danger of burning, in regard the Valve covered with Leather is put in afterward through the Elliptick hole II.

4. But this Engine is chiefly to be preferred before others on this account, because we immit several bodies into the Receiver, through the Elliptick hole, and so make many Experiments in highly-compressed Air.

ICONISME V.

An Instrument to distill in vacuo.

AA **I**S a Brass Vessel, shut below and open above. Fig. 1.
BB Is a Diaphragma or Midriff of Tin, whose edges are so polished on both sides that they exquisitely do agree and suit with the edges of the Vessels AA DD, which are also polished, and so keep the external Air from Ingress.

CC Is a Tube fastened to a hole in the middle of the Diaphragma BB.

DD Is a Brass Vessel whose aperture is applied to the Diaphragma BB.

EE Is a Stop-cock fastned to the hole of the Diaphragma BB.

FF Is a Tube reaching from the Stop-cock EE to the hole for suction in the Pneumatick Engine.

GG Is a metalline Vessel shutting in the commissures of the Vessels with the Diaphragma, and also the Stop-cock, that it, being filled with water, may keep all safe from the external Air. This Vessel is to be soldred to the Vessel AA.

We use this Engine after the following manner, Taking away the Diaphragma BB, we put the things to be boiled into the Vessel AA, and so set it in a convenient place, that it be not shaken, whilst it is evacuated, then putting on the Diaphragma BB and the Vessel DD, we put to the Pneumatick Engine, and making use of the Tube FF, the Air is pumped out of the Vessels, the Vessel GG being yet first filled with water. Then the Stop-cock is to be shut, and taking away the Tube FF, we may place the evacuated Engine on the Fire, and the Vapours ascending through the Tube CC, are condensed

densed in the upper Vessel, and so we have a liquor distilled *in vacuo*; and the quantity of the generated Air, is known by the Mercurial Gage H, but that must be kept up in the Top of the Receiver, lest the Mercury do exhale, by reason of too much heat.

Note that round pieces of Paper, perforated in the middle, are to be laid over the orifices of the Vessels AA DD, to the end they may be better joyned with the Diaphragma; and the commissures of the Tube FF with the Stop-cock and Pneumatick Engine are to be fortified with cement, and the Stop-cock EE is so to be disposed with the Vessel GG that part of the Key may be prominent without the Vessel through the hole, that so it may conveniently be turned, and yet nevertheless, the Stop-cock, with the Diaphragma, may be taken out of the Vessel GG, whilst the Vessel EE is to be filled with flesh or any other matter. And that is very easily done in this manner, The Key consists of two parts, one of which M is turned in the Stop-cock it self, by means of a certain chink, which receives the small protuberance of the other part OO, which other part doth exactly fill the small Pipe NN, fastned to the Vessel GG, and being prominent outwardly may easily be turned in it, and communicate its motions to the other part M, but it is drawn outward whilst the Diaphragma BB is to be taken out of the Vessel GG.

Fig. 2. Shews you another Instrument, herein differing from the former, that it is almost all of Glasse and affords a longer passage for the vapours.

BB Is not a Diaphragma, but onely a small Tube, polished at both ends, that it may exquisitely suit with the orifices of the Vessels A and D.

AA DD Are two Glasse Vessels, whose orifices are applied to the Tube BB, and so the Vapours are easily transmitted from the one to the other.

EE

EE FF GG I have the same Use as in the former Scheme, and the whole Instrument is to be evacuated after the same manner, and placed upon the Fire, except that here the Vessel AA, as being made of Glâs, must not be put on an open Fire, but *in balneo Mariæ*, or on Sand; and the Vapours will be condensed in the Vessel DD.

ARTICLE I.

Several waies used to help the Production of the Air.

EXPERIMENT I.

July 11. 1676.

BEcause it appears by the new Experiments published at Paris, in the year 1674. and which are to be sold by John Cusson in St. James Street, That Bread alone can produce no Air *in vacuo*, we were willing to try whether yet it did not contain some Air, which might come forth some other way. I therefore included a little Piece of Bread, very moist and a little kneaded, *in vacuo* with a Mercurial Gage.

July 12.

In six hours space no Air was produced yesterday, but this night a little brake into the Receiver, as much as did suffice to sustain three digits of Mercury; the reason was, because I had neglected to fortifie the Cover with Turpentine.

Towards the Evening, I found the Mercury higher by one inch or thereabout, and I am very certain that nothing had entred from without.

July 13.

This night also the Mercury ascended higher, but my Gage was not of that sort as exactly to discover many degrees.

The Second Continuation of

July 26.

This day the Piece of Bread disjoined its Receiver from the Cover, by the force of the produced Air, and the Smell of it was acid.

Hence it follows, That Water is a fit Dissolvent to draw forth Air out of Bread.

EXPERIMENT II.

July 11.

I tried another way to extract Air from Bread; for by the help of a Burning glass I burnt Bread *in vacuo*, and so I found that the Bread did generate much Air, and that Air did ever and anon break out, as by Fulmination; whence it seems probable, that Air is contained in Bread, but it is so closely coarctated therein, that no easie operation can give it a discharge; but if any thing could dissolve and loose that knot, it may then produce great effects.

EXPERIMENT III.

Sept. 22.

I took eight ounces of dried Grapes, and, with seven ounces of Water, included them in a Receiver, able to hold 22 ounces of Water, the Grapes were bruised.

Sept. 23.

The Receiver was demersed under the Water all this night, yet the Mercury ascended two whole inches.

Sept. 30.

In seven daies space, the Mercury came to the height of thirteen inches.

October 5.

In five daies space, the Mercury ran up twelve inches, and was now 25 inches high.

Octob.

Octob. 18.

The Mercury did not proceed to ascend with the same swiftness, and the Air began to pass out of the Receiver, but not before this day; yet these Grapes produced much more Air than those which I had included without Water. See Art. IX. Exper. I.

EXPERIMENT IV.

July 12.

I included of Raisins of the Sun bruised ten ounces *in vacuo*, with a sufficient quantity of Water to promote Fermentation.

July 14.

In 2 daies space the Raisins had produced ten inches of Air.

About the evening the Mercury was about fifteen inches high: the fifteenth day, the Mercury had almost reached to its accustomed height.

July 16.

This day, in the morning, I found the Receiver severed from its Cover, and the Air breaking forth through the Water, in which it was demerged: I included the same Raisins again *in vacuo*.

July 18.

This day, in the morning, I found the Air again breaking out.

July 19.

I shut up the same Raisins in the same empty Receiver.

July 21.

This day I found the Receiver full, and the Air breaking out of it.

I again shut in the same Raisins in the same exhausted Receiver.

July 23.

Yesterday about noon I found the whole Receiver almost full.

full of Air, and this day in the morning I perceived the Air to pass out very often. From the I. Experiment of *Artic. IX.* it appears; that Grapes, without Water, can generate but little Air: so that it is manifest hereby, that Water is a fit *medium* to elicit Air out of them: 'tis also evident that the Production of Air is not begun presently upon the Affusion of Water; but it proceeds on with greater swiftness, after that the parts of the Water in five or six days time have more deeply sunk into, and pervaded the Grapes.

EXPERIMENT V.

August 13. 1677.

I included Pears in two Receivers *in vacuo*; and Plums in another.

Aug. 16.

In three days space all my Receivers were filled with Air, newly generated; yea, one of them, which included the Pears, because I had left it exposed to the Raies of the Sun, in the space of 24 hours, was separated from its Cover, whence we may conjecture, that the Production of Air is very much promoted by the Heat of the Sun.

EXPERIMENT VI.

Octob. 16. 1677.

I took two ounces of Grapes bruised, and secured them from the ingress of Air, in an exhausted Receiver, capable of containing twenty ounces of Water.

Octob. 17.

The Mercury rose higher about one half inch.

Octob. 18.

These last 24 hours the Mercury ran up about another half inch.

Octob.

Octob. 20.

The height of the Mercury was two inches.

The 22 it was almost 4.

The 27 it was almost 6 inches.

Jan. 2. 1678.

The Mercury as yet came not to the height of 10 inches.

Octob. 16. 1677.

I put 3 ounces of bruised Grapes, with half an ounce of Spirit of Wine into a Receiver able to hold 30 ounces of Water, and then I exhausted the Air.

Octob. 17.

The Mercury ascended but a very little.

Octob. 18.

The Mercury came not up to the height of one quarter of an inch.

Octob. 20.

The Mercurial Gage was out of order.

Jan. 2. 1678.

I this day found my Receiver filled with Air, and also, when some of the Liquor was poured out, some Bubbles were formed in the Turpentine about the Orifice, and were broke outwardly.

From this Experiment, made in two Receivers together, it seems to follow, that Spirit of Wine doth much advance the Production of Air *in vacuo*, though in common Air, it wholly hinders it. See the II. VIII. and XIV. *Experiments* of the II. *Article*.

EXPERIMENT. VII.

July 19. 1678.

I put Must, expressed from Grapes bruised, and kept for 10 months in a Vessel, stopd with a Screw, into the same Receiver, being also stopd with a Screw.

E

July

July 21.

The Mercury had not ascended at all.

23. The height of it was 3.

24. The height was 5.

25. In the morning it was 104.

Towards the evening the height was 137; and the Must got out.

26. The Must was almost all got out of the Receiver; and although the Air now did possess double the space it did yesterday, yet it kept up the Mercury in the same height.

27. About half of the remaining Must brake forth this night, because I had omitted to *set* the Screw, lest the Receiver should have been broken in pieces.

From this Experiment it follows, that Grapes kept so long a time, do rather *acquire* than *lose* a fermentative Virtue.

EXPERIMENT VIII.

Jan. 30.

I put two quantities of Apples, boiled the day before, into two Receivers stopp'd with a Screw; with one of them I mixed one third part of Sugar, the other had no Sugar at all.

N. *All these Receivers were quite full.*

Jan. 31.

I included raw Apples bruised in three Receivers; in one of them I mixed one third part of Sugar; the second was without Sugar, and so was the third, but it differed herein from the second, that it was six times as big: For by this means we may know, whether the capacity of the Vessel, or the mixing of Sugar, or the crudity of the Fruit, can promote or retard the Production of Air.

Febr. 10.

In that Receiver onely which contained the raw Apples with Sugar some Air was produced.

Febr.

Febr. 14.

The raw Apples with Sugar had impelled the Mercury up to 30 inches; those that were boiled with Sugar, to two only; in the other Receivers no Air was produced.

Febr. 18.

In the Receiver, containing the raw Apples with Sugar, the Mercury came to the height of 56 inches; in that containing the boiled Apples with Sugar, the height was 3. in the other Receivers there was also some Air produced, except in that wherein the boiled Apples without Sugar were put. I opened that Receiver in which the Apples had produced so great a quantity of Air; yet the Apples seemed hardly to be fermented, but were endued with a most pleasant Taste.

Febr. 21.

The boiled Apples without Sugar had lost some of their Juyce; and, opening the Receiver, I found the Cover to be broke, and yet the Apples were not rotten at all.

March 1.

In the great Receiver, containing the raw Apples, the Mercury was 25 inches high; in the little one, onely 7; but in that where the Apples were boiled with Sugar, the Mercury had ascended to 9 inches.

March 8.

In the great Receiver the height of the Mercury was 29; in the lesser 22 $\frac{1}{2}$; and where the boiled Apples with Sugar were, the altitude abode at 9 digits.

March 17.

The Juyce got out of the great Receiver; in the little one the height was 67; where the Apples were boiled with Sugar, it was 15 digits.

From this Experiment it seems inferrable, that Sugar, the Crudity of the Fruit, and the Largness of the Receiver, do all contribute to the Production of Air.

ARTICLE II.

Several waies to hinder the Production of Air.

EXPERIMENT I.

Decemb. 21. 1678.

I made Paste of Bread-corn-meal, without Leaven, and put it into an empty Receiver, and then I put the Receiver in a certain Apartment, with Fire, which there kept a greater heat than is wont to be in the middle of Summer; yet the Dough or Paste produced no Air in 10 hours space; whence it seems to follow, that if Dough hath once suffered too much Cold, it can scarce recover its faculty of Fermenting; for, some years ago, when I made Dough without Leaven, in the Summer time it produced very much Air *in vacuo* in a short time.

EXPERIMENT II.

May 23.

I included 3 ounces of Dough, kneaded with Leaven, in a Receiver capable of holding 50 ounces of Water; I also poured upon it some quantity of Spirit of Wine, to try whether Fermentation would be hindred by that means.

May 24. The Mercury was 3
inches high.

26. Little change.

27. No change.

May 29. No change.

June 2. It seemed to have ascended a little higher.

14. No change.

Decemb. 14.

No more Air being produced from the Dough, I took it out from the Receiver, and found the smell of it not gratefull, but subacid: I put it into an empty Receiver, and there it rose or swelled to double its accustomed space, and made a little Ebullition.

May

May 23.

I included 3 ounces of Dough kneaded with Leaven in a Receiver able to hold 50 ounces of Water, but here I mixed no Spirit of Wine.

May 24. The Mercury was 19 $\frac{1}{2}$ inches high. *May 26.* 'Twas 38 inches high.
27. There was no change.

Dec. 14.

The Mercury persisted in the same height; and this day, opening the Receiver, I found the Dough of a most acid smell.

From which Experiment it seems to follow, that Spirit of Wine, even in Dough kneaded with Leaven, doth hinder the Production of Air.

EXPERIMENT III.

August 29.

I included Pears, with a Mercurial Gage, in a Receiver full of Water, and then I intruded Air into it, till the Mercury staid at 26 inches higher than it was wont; within a quarter of an hour, one of the Pears was broken, and afterwards almost all of it was reduced to the consistence of a Pultis.

Aug. 30.

In 24 hours space, the Pears seemed to have afforded no Air; but on the contrary, the Mercury in the Gage was depressed an inch and half.

Aug. 31.

I this day found no change in the height of the Mercury.

Sept. 1.

Now the Pears began to produce Air, and the Mercury was almost 27 digits high.

Sept. 2.

In 24 hours the Mercury ascended more than 8 digits, and now 'twas 35 digits high.

Sept. 3.

The height of the Mercury was increased 17 digits, so that now it was 52 digits high or thereabout.

Sept. 4.

Within those 24 hours the Mercury rose 7 digits higher, and rested then in 59.

Sept. 5.

It was 64 digits high; a Pear, being broken, was become black.

Sept. 6.

Three digits and more being added to the height of the Mercury, it came now to the 67 digits and $\frac{1}{4}$ beyond what it was accustomed.

Sept. 7.

It descended 3 digits, and rested again in 64.

Sept. 8.

This day the Mercury was depressed to the 58 digit, and some of the Water had broke out; and therefore I straitned or *set* the Receiver with a Screw.

Sept. 9.

The Mercury ascended full 3 digits, and now stuck suspended above 67.

Sept. 10.

In 24 hours it mounted $1\frac{1}{2}$, and stopped almost in 69.

Sept. 11.

Now it began to descend again, and was no higher than 67 digits; yet I am certain, nothing had escaped out of the Receiver, but it was a sharp cold night.

Sept. 12.

No change did evene.

Sept. 13.

The height of the Mercury did again decrease; it was not above 64 digits: the Cold increased.

Sept.

Sep. 14.

In 24 hours it became higher by 6 digits, reaching to 70.

Sept. 16. It was 69 digits high, | Sept. 20. It again reached to
or thereabouts.

19. It remained in the
same place.

71.

23. The Mercury was a-
gain depressed to 69.

Octob. 1.

It came now to the height of 75 digits.

Octob. 3.

Yesterday I found no change at all in the Mercury; but this
day it stuck in 70; and the Cold was very bitter.

Octob. 5.

Yesterday the Mercury did abide in the same place; but
this day it reached to 75: it was a rainy day.

Octob. 7.

It continued rainy; and the Mercury continued in the same
place.

Octob. 10.

Hitherto the Mercury was not changed; but this day I
found it had descended to 69 digits; though the Rain ceased not.

Octob. 12.

Yesterday the Mercury stood still; but this day it was de-
pressed to 65 digits: and the cold weather returned.

Octob. 13. The height of the Mercury was 64.

14. } The height } 69.

15. } was } 74.

24. The height was 68.

It was a cold season.

Nov. 2. The height was 64.

The Cold encreased.

Nov. 5. The height was 80½.
The Cold abated.

2. The height was 65.

It was a hard Frost.

27. The height was 68.

It was a Thaw.

Decem. 6. The height was 61.

It was a very bitter Frost.

From the former Experiment we may learn, That Fruits in
a great Compression of the Air, cannot produce so great a
quantity of Air; for when I made an estimate of the quantity
of

of the Fruits, and of the small space which is to be filled with Air; I found, that that quantity of Air was not $\frac{1}{3}$ part of that which had been produced in an empty and a large Receiver: yet the Cold of the Water might also give some Impediment to the Generation thereof, as the following Experiment will confirm.

'Tis also farther manifest, that the Air is produced by iterated turnes, and as it were by reciprocations, even as all bodies in motion by the force of their gravity or of their spring are carried beyond their point of rest, and so suffer many vibrations, or goings and returnings: Now although Cold and Heat are not the sole causes of such reciprocations, yet they seem to contribute much thereunto.

EXPERIMENT IV.

Febr. 22. 1677.

I included 10 ounces of Paste in a Receiver capable of holding 22 ounces of Water, and afterward I thrust as much Air into it as was sufficient to sustain 73 digits of Mercury, besides the wonted Pressure. In two hours space I perceived no sensible change.

Febr. 23.

In 18 whole hours the Mercury ran up 7 digits onely, its height being 80.

In 6 hours space it was now ascended 3 digits; its height was 83.

<i>Febr. 24.</i>	} Its height was	90	And Water seemed to be expressed out of the mass.
25.		97	
26.		101	
27.		105	
28.		107 $\frac{1}{2}$	
<i>March 1.</i>		112	
			<i>March 2.</i> } Its height { 120
			3. } was { 121
			4, & 5. It stayed at 121

March

March 8.

These 2 or 3 last daies, the Frost being dissolved, the Mercury ran up 4 digits: the height thereof was 125.

March 10.

Yesterday the Mercury persisted in the same height; but this day, mounting 6 digits, it stayed in 131.

March 21.

By reason of the long cold season, no Air was produced: but in the three last daies the Mercury ascended 7 digits, and stayed in 138.

April. 4.

Yesterday I perceived the Mercury had ascended, but I deferred exactly to measure the quantity till this day: But in this very night one of the Iron-wires, that straitned the Receiver was broken, and so the Receiver was ejected to 4 or 5 foot distance.

From this Experiment we may conjecture, that the Compression of the Air did very much hinder the Production thereof; for *that* is wont to be perfected in Paste in 2 or three daies space. Moreover, Cold doth much hinder the same Production.

EXPERIMENT V.

March 1. 1677.

I included two ounces of Raisins of the Sun with six ounces of Vinegar in an empty'd Receiver, and Bubbles in a sufficient quantity did break forth: the Raisins were bruised.

March 2.

The Mercury in 24 hours space ascended not to the height of half a digit: yet some Bubbles still appeared.

March 25.

The Vinegar did alwaies appear interspersed amongst some of the Bubbles, yet the Mercury ascended not to the height of one digit.

F

By

By this Experiment it appears, That Vinegar doth hinder the Production of Air and Fermentation; seeing otherwise Raisins are wont to afford much Air.

EXPERIMENT VI.

Apr. 7. I included 10 ounces of Paste in a Receiver capable of holding 22 ounces of Water; afterwards I intruded Air into it, as much as sufficed to sustaine 128 digits of Mercury, besides its accustomed height.

In 6 hours space the Mercury mounted up 4 digits, and staid in 132.

Apr. 8. In 16 hours the Mercury ran up 9 digits higher; it staid in 141.

Nine hours after the Mercury was not changed.

Apr. 9. This day, in the morning, I perceived some Air had broke forth, and the Mercury was depressed to 130 digits, and therefore with a Screw I shut the Receiver more closely, and thrust in 11 digits of new Air: the height was 141.

<i>Apr. 10.</i>	$\left. \begin{array}{l} 11. \\ 12. \\ 13. \end{array} \right\} \begin{array}{l} \text{The height} \\ \text{was} \end{array} \left\{ \begin{array}{l} 151 \\ 158 \\ 168 \\ 176 \end{array} \right.$	<i>Apr. 14.</i>	$\left. \begin{array}{l} 15. \\ 16. \\ 17. \end{array} \right\} \begin{array}{l} \text{The height} \\ \text{was} \end{array} \left\{ \begin{array}{l} 183 \\ 183 \\ 187 \\ 191 \end{array} \right.$

Apr. 27.

For eight whole daies the Mercury kept its station in the same place, but these two last daies it ascended 7 digits, and stayed in 198 above its wonted height.

Apr. 30.

Perceiving the Mercury to persist in the same height, I a little relaxed or eased the Screw, that some Air might break forth; and when I saw that the Mercury had so far descended, that it exceeded its accustomed height onely 50 digits, I presently set the Screw, that so I might know whether that remission of the Spring of the Air would afford any place for new Air to be generated; and truly in two or three minutes time

time I found the Mercury to have ascended sensibly higher.

Three hours after, making an Admeasurement, the Mercury was found 12 digits higher; for it came to 62.

In 5 hours space it ascended 1 digit and $\frac{1}{2}$ and no more.

May 1.

In 15 hours the Mercury gat higher onely one digit.

May 3.

Yesterday the Mercury persisted in the same height, but this day 'twas higher by $1\frac{1}{2}$, and remained in 66.

May 4.

The Mercury was not changed at all, and therefore I suffered all the Air to escape; but something hindred, that I could not quickly *set* the Screw, whence it is probable, that very much Air, which at that time was produced, got out of the Receiver; yet nevertheless, after the Receiver was again straitly stopp'd, I perceived that two digits of Air and more had been produced in 5 or six minutes time.

May 7.

The Mercury in 3 daies, again mounted 2 digits.

May 8.

The Mercury was higher by $\frac{1}{2}$ a digit.

May 11.

Those two last daies the Mercury again ran up half a digit, and not much more. I included this mass, almost unfit, as it seemed, for producing of Air, *in vacuo*; and then in 5 hours space the Mercury ascended to the height of one digit.

May 21.

It did not yet ascend quite 3 digits.

May 30.

The Mercury staid at the height of 4 digits and $\frac{1}{2}$.

By this Experiment it appears, that all the Air producible from Paste, may be in a manner generated in a great Compression; yet it is somewhat restrained by that hindrance, which at length in a lesser Compression will break forth in a short time.

Moreover, we have a confirmation by this Experiment, that Air is producible by repeated turns and operations; also, that it is produced more slowly in compressed than in free Air: For such a Production in free Air is wont to be perfected in two or three daies time.

EXPERIMENT VII.

July 30. 1677.

Artificial Air.

I included Plums and Apricocks, many of them being cut asunder, in an empty Receiver, and afterwards I immitted as much Air, produced out of Cherries, into the same Receiver as was sufficient to sustain 64 digits of Mercury.

Aug. 1.

Our Fruits had produced no Air, but grew yellow by reason of their overmuch Ripeness, more than those which were in Common Air. See p. 37.

Aug. 3.

This day I found the Mercury a little higher, and that Apricock which remained whole, seemed to be full of some drops of Water.

Aug. 7.

The whole Apricock grew more and more soft; the Mercury was 59 digits high above its wonted Pressure.

<i>Aug.</i>	8.	$\left\{ \begin{array}{l} \text{The height} \\ \text{of it was} \end{array} \right\}$	61	$\left\{ \begin{array}{l} \text{The height} \\ \text{of it was} \end{array} \right\}$	78	
	9.		65		13.	80
	10.		71		14.	80
	11.		74		15.	80

16. and the days following it abode at the same height.

24. The height of it was 77. Though I certainly knew that nothing had issued or escaped out of the Receiver.

29. Seeing I found that neither the Fruits nor the height of the Mercury were changed any more, I opened the Receiver

Receiver and perceived that the Apricocks had kept their colour very well, but the flesh of them was spongy, and their taste subacid; many bubbles had broke forth from them, at the time they were freed from the *circumstant* pressure.

July 30. 1677.

Common Air.

I included the half parts cut off from the Fruits aforesaid, in a Receiver full of Common Air; and with them also some Fruits of the same kind uncut.

July 31.

I found the Mercury had attained 8 digits high.

August 1.

At 6 a Clock in the Evening the Mercury was 21 digits high; in the other Receiver it was not moved.

August 3.

Our Fruits kept their firmness much better than those which were included with Artificial Air. The height of the Mercury was 35 digits.

August 4.

The height of the Mercury was 42 digits.

August 6.

Our whole Apricock seemed not at all to be altered. The height of the Mercury was 57.

Aug. 7	} The height	81		Aug. 9	} The height	113
8				10		
		95				

The colour of the whole Apricock yesterday began, and now proceeded to wax yellow. No moisture appeared.

Aug. 11	} The height	{ 131		Aug. 15	} The height	{ 171		
13				16			} of it was	{ 171
14				17				
		157						
		163						

F 3

Aug.

Aug. 27. The height was 182.

29. When I saw that neither the Fruit nor the height of the Mercury were changed any more, I opened the Receiver, and found the Apricocks of a more acid and less acceptable taste, than the others in factitious air; yea, their pulp was of a very good colour, but spongie: they sent forth many bubbles, as the others did.

From this Experiment made in two Receivers together, 'tis probably collected, that the artificial Air of the Cherries was a great hindrance to the Apricocks, that they could not produce air; yet notwithstanding, it doth advance the alteration of their colour and firmness; and is also good to preserve their taste.

EXPERIMENT VIII.

Octob. 10. 1677.

Grapes without spirit of Wine.

I shut in an ounce and half of Grapes unripe and bruised, in a Receiver that would hold 10 ounces of Water; I drew out no Air.

Octob. 11. The Mercury ascended a little.

12. There was but a small change.

13 The height was $\frac{1}{2}$ a digit.

17 The height was 1 digit.

18 The height 1 $\frac{1}{2}$

19 The height almost 4 digits.

20 The height the

same, but some finew or mouldiness appeared in their superficies.

21 The height was 4 $\frac{1}{2}$

22 } The height remained the same,
23 }
24 } but the mouldiness or finew encreased.

26 } The height 5 $\frac{1}{2}$

27 } of it was 6

30 } 6 $\frac{1}{2}$

Nov. 2 } 7 $\frac{1}{2}$
Nov.

Nov. 6	$\left. \begin{array}{l} 8 \\ 9 \\ 12 \\ 14 \end{array} \right\} \begin{array}{l} \text{The height} \\ \text{of it was} \end{array} \left\{ \begin{array}{l} 9 \\ 10 \\ 12 \\ 15 \\ 17 \end{array} \right.$	Nov. 18	$\left. \begin{array}{l} 21 \\ 8 \\ 12 \\ 27 \end{array} \right\} \begin{array}{l} \text{The height} \\ \text{of it was} \end{array} \left\{ \begin{array}{l} 23 \\ 26 \\ 36 \frac{1}{2} \\ 39 \\ 39 \end{array} \right.$
8		21	
9		8	
12		12	
14		27	

Jan. 6. 1678. The height was 36. The air broke out.

Octob. 10. 1677.

Grapes with spirit of Wine.

I made the same Experiment in another Receiver, observing the same circumstances, save that here I mixed 2 drachms of spirit of Wine with the Grapes.

Octob. 11. The Mercury was	not changed.	Oct. 17. It ascended a little.
12. There was no change.		18. The height of it was not yet a quarter of an inch.
13. The Mercury was	not moved.	19. It was moved but a very little.

Jan. 6.

The Grapes during all the time elapsed, had produced no air.

By this Experiment made in a double Receiver, it appears that spirit of Wine doth hinder Fermentation.

EXPERIMENT IX.

Octob. 17. 1677.

I put one Peach into an emptied Receiver, with some quantity of spirit of Wine, which yet could not touch the Peach, unless it were elevated into vapours.

March 27. 1678.

I drew out the Peach, which had kept its colour, onely it had lost its firmness. Though the Receiver was but small, yet it was not filled with air, for when it was opened, the air seemed

to

to rush into it: The Peach being softened, was so depressed, that the lower part of it did a little touch the spirit of Wine; it also came to pass, that the superiour part had almost contracted the taste of the spirit of Wine, as well as that which was immersed in it.

E X P E R I M E N T X.

Octob. 17.

Air with spirit of wine.

I included 5 Peaches in an unexhausted Receiver, and together with them, some spirit of Wine, which could not touch the Peaches, unless it were elevated in form of Vapours.

<i>Octob. 18.</i>	The Mercury ascended not at all.	<i>Nov. 6</i>	The height of it was	digits 14 16
20.	The height of the Mercury was $3\frac{1}{2}$	12	It kept the same height.	16
21	The height of it was	5	The height of it was	18
22		7		19
23		9		20
26		9		21
<i>Nov. 2</i>		12		
		<i>Dec. 8</i>		
		16		
		27		
		<i>Jan. 6. 1678.</i>	it was	23
		<i>March 28. 1678.</i>	it was	31

Octob. 17.

Air without spirit of Wine.

I included 5 Peaches in a Receiver full of Common Air, without spirit of Wine.

Octob. 18.

The Mercury ascended not at all

Octob. 20.

The height of the Mercury was 5 digits.

Octob.

		digits				digits
Octob.	21	$\left. \begin{array}{l} 8 \\ 10 \\ 11 \\ 12 \end{array} \right\} \begin{array}{l} \text{The height} \\ \text{of it was} \end{array}$	Nov.	12	$\left. \begin{array}{l} 14 \\ 16 \\ 16 \\ 27 \end{array} \right\} \begin{array}{l} \text{The height} \\ \text{of it was} \end{array}$	20
	22			14		20
	23			16		21
	26		Decemb.	8		26
Nov.	2	$\left. \begin{array}{l} 15 \\ 17 \frac{1}{2} \end{array} \right\}$		16	$\left. \begin{array}{l} 26 \frac{1}{2} \\ 28 \frac{1}{2} \end{array} \right\}$	26 $\frac{1}{2}$
	6			27		28 $\frac{1}{2}$

Jan. 6. 1678. The height was 32

March 28. 1678. The height was 33 $\frac{1}{2}$.

April. 15.

The Liquor in the lower part of the Receiver had broke all out, and the air followed it; so that I took out the Peaches.

By this Experiment we learn, That the very Vapours of spirit of Wine do somewhat hinder fermentation; yet much less than the spirit it self.

EXPERIMENT XI.

April 27. 1678.

Paste with Leaven or Ferment.

I included an ounce and half of Paste, mixed with leaven with common air in a Receiver, able to hold 23 ounces and half of water.

April 28.

The height of the Mercury in the Gage was 2 $\frac{1}{2}$.

April 30.

The height of it was 3 $\frac{1}{2}$.

May 4.

The Mercury was depressed, though no air broke forth, and the Paste was mouldy. The height of it was 2 $\frac{1}{2}$.

May	6	$\left. \begin{array}{l} 8 \\ 10 \\ 14 \end{array} \right\} \begin{array}{l} \text{The height} \\ \text{of it was} \end{array}$	$\left. \begin{array}{l} 2 \frac{3}{4} \\ 3 \\ 3 \frac{1}{2} \\ 4 \end{array} \right\}$	May	17	$\left. \begin{array}{l} 20 \\ 24 \\ 28 \end{array} \right\} \begin{array}{l} \text{The height} \\ \text{of it was} \end{array}$	$\left. \begin{array}{l} 4 \frac{1}{2} \\ 5 \\ 6 \\ 8 \end{array} \right\}$
	8				20		
	10				24		
	14				28		
				G			June

The Second Continuation of

<i>June</i>	2 6 14	} The height of it was	$\left. \begin{array}{l} \text{digits} \\ 9 \\ 10 \\ 10\frac{1}{2} \end{array} \right\}$		<i>July</i>	5 19	} The height of it was	$\left. \begin{array}{l} \text{digits} \\ 13\frac{1}{2} \\ 15 \end{array} \right\}$

*April 27. 1678.**Paste without Leaven.*

Included an ounce and half of Paste, without Leaven, with common air, in a Receiver capable of holding 23 ounces and an half of Water.

April 29.

Hitherto the Mercury had not ascended; but this afternoon I found its height to be a quarter of a digit.

April 30.

There was no change.

May 4.

The Mercury ascended but very slowly, and the Paste was finewed or mouldy.

May 6.

The height of the Mercury was 4 digits.

<i>May</i>	8 10 14 17 20	} The height of it was	$\left. \begin{array}{l} 5\frac{1}{2} \\ 7\frac{1}{2} \\ 10\frac{1}{2} \\ 12\frac{1}{2} \\ 13\frac{1}{2} \end{array} \right\}$		<i>May</i>	24 28	} The height of it was	$\left. \begin{array}{l} 16 \\ 18\frac{1}{2} \\ 20\frac{1}{2} \\ 21\frac{1}{2} \\ 25 \end{array} \right\}$

By this Experiment, made in two Receivers at once, it seems clear, That Leaven doth rather hinder than help the production of Air, if the Paste be not made in a place hot enough.

EX.

EXPERIMENT XII.

May 23.

Paste with spirit of wine.

I included an ounce and half of Paste, without Leaven, in a Receiver capable of holding 25 ounces of Water, and I poured spirit of wine on the Paste.

May 24. The Mercury was 1 digit high.

May 26. It was almost 2 digits high.	June 1	} The height of it was	{ $3\frac{1}{2}$ $4\frac{1}{2}$
27. It was $2\frac{1}{2}$.	6		
31. There was no change.	10		
	July 19	No change.	

December 14.

When the height of the Mercury was no more changed, I opened the Receiver, and the Paste affected my Nostrils with a subacid smell.

May 23.

Paste without spirit of wine.

I included one ounce and an half of Paste, without Leaven, in a Receiver capable of holding 25 ounces of Water; but I added no spirit of Wine.

May 24.

There was no ascension of the Mercury.

May 26. It was 3 digits high.

May 27	} The height of it was	{ $4\frac{1}{2}$ $5\frac{1}{2}$ 7 $9\frac{1}{2}$ 12	June 6	} The height of it was	{ 17 22
28			10		
29			July 4	} The Mercury little exceeded 30 digits. This day	
31			July 19		
June 2					

day I found that the Air had broke out, and therefore I *set* or *Aratined* the Screw.

December 14.

The Mercury came again to the height of 15 digits, but this day I opened the Receiver, and found the Paste very acid.

From these Experiments, made with Paste, in a four-fold Receiver at one and the same time, it seems to follow, That spirit of Wine doth very much prejudice the production of Air; and the rather if the Paste be wrought with Ferment; besides, it is clear, that Paste *without* Ferment in tract of time, will produce no less Air than Paste *with* Ferment:

EXPERIMENT XIII.

Octob. 11.

I included new Ale in a Receiver, exactly filled by the help of my Pneumatick Engine, that so no air might be left: And I included another quantity of the same Ale, in another Receiver, wherein some room was allowed for the Air.

Octob. 12.

I this day found the Cover of that Receiver in which I had left some Air, to be broken, and therefore I transfused the same Ale into another Receiver, in which there was room large enough left for the Air. In the Receiver exactly full, the Mercury ascended a little,

October 13.

In the Receiver exactly filled, the height of the Mercury was 12 digits, in the other Receiver 13 digits, though it had been shut up a shorter time, and a much larger space was left therein, in which the Air newly produced might have been dilated.

October 14.

In the full Receiver the height was 13; in the other Receiver, 18. Towards Evening I found the full Receiver to work

work with greater swiftness, for the height of the Mercury in it, was 22; and in the other 20.

October 15.

In the full Receiver the height of the Mercury was 42 digits; in the other 26. Besides we must mark, that some bubbles of Air, which in the full Receiver had possessed its upper part, now did wholly vanish; and besides the Ale did occupy a long space in the Mercurial Gage, wherein before it was not found.

October 16. In the full Receiver the height was 60 digits.

In the other 30.

18. In the full Receiver the height was 90.

In the other 40.

22. In the full Receiver the height was 90.

In the other 42.

23. In the full Receiver the height was 108.

In the other 50.

26. In the full Receiver the height was 108.

In the other 60.

28. In the full Receiver the height was 133.

In the other 63.

The bubbles which were vanished, appeared again, yet nothing flowed out.

Nov. 8.

The full Receiver had lost much Ale, wherefore I opened it, and thereupon all the Ale seemed as if it would have vanished into Froth, unless I had suddenly shut the little hole, which I had opened: I tried it many times, that if the hole were opened in the Gage, the Mercury presently descended; but if the hole were again shut, it would speedily ascend; as if the compression, being abated, had afforded some facility for the production of Air. The Ale had a most pungent taste.

Nov. 9.

I opened the other Receiver, and observed in a manner the same circumstances.

From

From this Experiment it seems to follow, That Ale if the Air be wholly excluded from the Vessel will ferment more slowly than if some Air were left with it: yet in tract of time, it makes a greater compression, if no place be left for its dilatation.

E X P E R I M E N T X I V .

June 27.

Pease with spirit of wine.

I put green Pease into an emptied Receiver, with spirit of Wine. Towards the Evening the Receiver seemed to admit the external Air, and the Mercury came to the height of 18 digits; and therefore I firmed the Cover with Turpentine.

June 30.

I perceived no more change in the height of the Mercury.

July 7.

No Air was produced, even in the most vehement heat.

June 27.

Pease without spirit of Wine.

I put new Pease into an emptied Receiver, without spirit of Wine. The Receiver and the quantity of the Pease were the same, as in the last mentioned Experiment.

June 28.

The Receiver was full of Air, for I think it was not exactly shut; and therefore I again included the same Pease. Towards Evening the height of the Mercury was 5 digits.

<i>June 29</i>	} The height {	10		<i>July 5</i>	} The height {	26
<i>30</i>		16		<i>7</i>		30
<i>July 1</i>		19				

of it was

July 8. The Air got out of the Receiver being too much filled.

From

From this Experiment, made in two Receivers at once, it appears, That spirit of Wine doth also hinder the production of Air in Peafe.

ARTICLE III.

The Effects of Artificial Air are different from the Effects of Common Air.

EXPERIMENT I.

June 19. 1677.

I Put Cherries into an evacuated Receiver. In 6 hours time the Mercury came to the height of 5 digits and an $\frac{1}{2}$.

June 20.

The ascension of the Mercury was $3 \frac{1}{2}$.

Towards the Evening it was 2.

N. *The Ascensions are always to be understood, as added to the former.*

June 21 }
22 } The ascen- { $1 \frac{1}{2}$
23 } sion was { $1 \frac{1}{2}$
24 } { 2
25 } { $1 \frac{1}{2}$
 { $1 \frac{1}{2}$

June 26 }
27 } The ascen- { 3
28 } sion was { 3
30 } { 5
 { $1 \frac{1}{4}$

July 1 }
2 } The ascen- { 3
3 } sion was { 4
 { 2

July 4 }
5 } The ascen- { $2 \frac{1}{2}$
 { 3
 { 3
 The height was 48; but

I transmitted the Air into another Receiver, and the Mercury was depressed to the height of 35 digits.

July

July 6. The ascension of the Mercury was 4 digits in one nights space.

7. The ascension of it was $5\frac{1}{2}$ in 24 hours space.

8. The ascension of it was 5.

9. The ascension of it was 5.

10. The ascension of it was 6.

11. The ascension of it was 12. in the space of 34 hours.

12. The ascension of it was 7.

13. The ascension of the Mercury was 3. the height about 92 digits; but the Air being transmitted into another Receiver, the Mercury staid in the height 50.

14	} The ascen- sion was	14		16	} The ascen- sion was	13
15		11		17		5

18. The ascension of the Mercury was 9. the height of it 102.

19. The height of the Mercury was 92. viz. because I transmitted part of the Air into another Receiver.

20 The ascension of the Mercury was 15.

22. Some Air got out, and the height of the Mercury was $63\frac{1}{2}$.

23. The ascension of it was $12\frac{1}{2}$.

24. The ascension of the Mercury was 4. the height of it was 79 digits; but the Air being transmitted into another Receiver, the height staid at 62.

25	} The ascen- sion was	8		27	} The ascen- sion was	4
26		9		28		5

30. The ascension of it was 10. the height was 98. Part of the Air being transmitted into another Receiver, the height staid at 64.

31. The ascension was 6.

Aug. 1. The ascension of the Mercury was 9. digits.

2. The ascension of it was 4.

3. I transmitted the Air into another Receiver, and the Mercury abode in the height 68.

- Aug.* 4. I transmitted the Air again into another Receiver, and the Mercury rested in the height 54.
 6. The ascension of the Mercury was 7.
 7. The ascension of it was 4.
 8. There was no ascension thereof.
 9. The ascension thereof was 3 digits.

The Receiver being opened, I found the Cherries of a whitish colour, and of very little taste; but the taste they had, was not ungrateful: their flesh or pulp was spongie.

From this Experiment it seems to follow, that Cherries contain much Air in them, and that they produce it very irregularly.

EXPERIMENT II.

July 13. 1677.

I put Cherries into an empty Receiver, and then I transmitted into the same Receiver, as much Air produced from other Cherries, as was sufficient to sustain 50 digits of Mercury.

July 15.

Yesterday the Mercury had not ascended at all; but this day it was two digits higher, *viz.* in 22 above its wonted height.

July 16. The height of the Mercury was $23\frac{1}{2}$.

July 17 The height of it | Mercury was 45. Some
 was 25. | more Air made an escape.

26. The height of it | 30. The height of it
 was 43. Some Air got | was 52.

out. | 31. The height of it
 27. The height of the | was 61 digits.

August 1.

The height of the Mercury persists in a manner the same, but the Air brake out.

H

August.

August 27.

The Air had all broke out for some time before; I took out the Cherries, and found them not to have lost their colour, as they had in the former Experiment; and besides they had contracted no putrefaction nor mouldiness, but had a taste a little more acid than they were wont to have; and being opened, there were many cavities in their pulp, like fermented paste or dough, but not quite so thick.

From this Experiment compared with the former, it may probably be inferred, that in Artificial air, fruits do produce less Air, and so they keep their colour and their taste better; for the Cherries in the former Experiment remained included in a Receiver, not much longer than those in *this*.

E X P E R I M E N T III.

September 10. 1677.

Common Air.

I put 6 ounces of unripe Grapes into a Receiver, capable of containing 25 ounces of Water; and I stop'd it firmly by the help of a Screw, with Common Air.

September 11. The Mercury ascended not at all.

September 12. The Mercury stop'd a little below one digit.

Sept. 13	} The height of it was	$\left\{ \begin{array}{l} 3\frac{1}{2} \\ 7 \\ 10 \\ 12\frac{1}{2} \\ 14 \end{array} \right.$	Sept. 18	} The height of it was	$\left\{ \begin{array}{l} 16 \\ 18 \\ 20 \\ 22 \\ 23\frac{1}{2} \end{array} \right.$
14			19		
15			20		
16			21		
17			22		

September 23. The height of it was 27. The Grapes were not altered.

September 24. The height was 30.

25. The height was 31. The Grapes now began to be yellow.

Sept.

Physico-Mechanical Experiments.

31

Sept. 26 } The height } $32\frac{1}{2}$ | Sept. 29 } The height } 35
 27 } of it was } 34 | 30 } of it was } 35

October 1. The height remained at 35.

Octob. 2. The height was 36 | Octob. 10 The height was 35
 5 } The height stayed | 13 The height of it was
 6 } at 36. | $32\frac{1}{2}$. The Air got not

forth, but the Cold began to come on and encrease.

Novemb. 9. The same height remained.

Decemb. 19.

I found the Air almost all to have made an escape.

Decemb. 20.

I took out the Grapes, and I found that by their Smell and their Taste, they had contracted some mouldiness, though the same was not discernable by the eye. Their firmness was encreased.

Septemb. 10. 1677.

Fallitious Air.

I included two ounces of crude Grapes in a Receiver capable of holding 8 ounces of Water; and to the Common Air, I superadded Air produced out of Pears, until the Mercury did stay 10 digits above its wonted pressure.

Septemb. 11.

The Mercury descended, its height was 8 digits.

Septemb. 12.

The height of it was 11. the ascension of it was 3.

Sept. 13 } The height } 16 | Sept. 1 } The height } 23
 14 } of it was } 20 | 16 } of it was } 24

Septemb. 17. The height was 28. the Grapes turned yellow.

Sept. 18 } The height } 29 | Sept. 22 } The height } 35
 19 } of it was } 30 | 23 } of it was } 20
 20 } of it was } 31 | Because some air had broke
 21 } of it was } 33 | out: The Grapes were also
 of a Yellow colour.

H 2

Sept.

Sept. 24. The height of the Mercury was 21 digits.

25. The height was 22.

26. The height almost the same.

27. The height abode in 22.

29. The height was 27.

30. The height was 28.

Octob. 1 & 2. The height stay'd at 28.

Octob. 5 } The height { 30 | *Octob.* 10 } The height { 31½
6 } of it was { 31 | 13 } of it was { 31

Novemb. 9. The height was 13. Some Air had got out.

December 19. The height of the Mercury was 20 digits.

Decemb. 20.

I took out the Grapes, and their Smell and Taste were more grateful than of others, and their Firmness was rather increased than diminished.

By this Experiment, made in two Receivers at once, we learn, That Factitious Air seems fit to alter Colour, and to preserve Taste; but the Firmness might be increased here, as it is augmented in Turpentine; *viz.* the Spirits in tract of time being exhaled.

EXPERIMENT IV.

July 18.

I took two pieces of Orange, and by the help of my Screw I stopped them in fast in my Receiver, with Common Air, and then into the same Receiver I put Air, produced out of Cherries, as much as was sufficient to sustain 12 digits of Mercury. At the same time I put another piece of the same Orange into another Receiver, with common Air alone, and that not compressed.

July 20.

The Orange in the common Air began to contract mouldiness; the other seemed not at all to be altered.

July

July 23.

The mouldiness of the Orange in the common Air increased; the other remained sound.

July 16.

The Orange in the common Air, did not proceed to increase its mouldiness, but seemed wholly rotten: the other also began to putrify, but remained free from mouldiness.

Aug. 1.

Perceiving that the Oranges were no more sensibly changed, I opened the Receivers, and though the Air, wherewith I had mingled artificial Air, was so compressed in its Receiver, that now it could not sustain 26 digits of Mercury above its wonted pressure, yet the Fruits were far better preserved in it, than in the other; onely something in the superficies seemed to have lost its juice, but all the inner parts, with the Rind, or Pill, were very well-coloured, well-tasted, and firm: In the other Receiver, the whole Orange seemed almost rotten, not excepting the Rind. In the *Exper. X. of Artic. IV.* the Orange was more corrupted in the compressed Air, because as it seems, no factitious Air had been mixed with it.

Here also it seems worthy our observation, That the same Air, generated from Cherries, is apt to produce different effects, upon Fruits of a different kind; for here it retarded the alteration of colour and firmness, which in *Exper. VII. of Artic. II.* where I included Air with Apricocks, it accelerated and hastened.

EXPERIMENT. V.

July 20. 1676.

Factitious Air.

I included a small piece of Beef in an emptied Receiver, and then I put Air, produced from Cherries, into the same Receiver, as much as sufficed to sustain 27 digits of Mercury.

July

July 21 }
 22 } The Mercury persisted almost in the same height,
 23 } and came not to its wonted pressure.
 25 }

July 26. This day the Beef had removed the Receiver from its Cover; and because it stunk very much, we threw it away.

July 20. 1676.

Common Air.

I put a piece of Beef into a Receiver full of Common Air, and I carefully stopped and firmed it in, by the help of the Screw.

July 21. The Mercury had not at all ascended in the Gage.

July 22. The height of the Mercury was 1 digit.

23. The height of it was $5\frac{1}{2}$.

25. The height of it was $9\frac{1}{2}$.

26. The height of it was $14\frac{1}{2}$. In the Evening 18.

27. The height of it was $21\frac{1}{2}$. In the Evening 25.

28. The Screw, not being firm enough, suffered the Air to break forth.

By this Experiment, made in 2 Receivers at once, it appears That Air produced from Cherries, is a great hindrance to the production of Air from Flesh.

EXPERIMENT VI.

March 14. 1676.

Common Air.

I put two Onions into a Receiver, full of Common Air, with a Mercurial Gage; and I fastned the stopple with a screw, to see whether Vegetation would increase the quantity of the Air, or diminish it.

March

March 28.

Two days after, the Mercury seemed depressed $\frac{1}{4}$ of a digit; but afterward it recovered its former height, and 2 digits more; and now the Air brake forth, and the Roots grew longer.

April 28.

About 10 or 12 days since I perceived the Roots to be corrupted; and indeed now they were wholly putrified.

May 9.

The Mercury persisted in the same height, because the Air had broke forth; and therefore I took out the Onions, and found their Roots putrified, but they were not mouldy at all.

March 17. 1676.

Facitious Air.

I included two Onions in an empty Receiver, and afterward put Air, produced from Paste, into the same Receiver.

March 28. My Onions took root, at least as well, as those which I kept in the Common Air.

April 28.

The ends of the Roots began to putrifie, yet they were in far better case, than those who are surrounded with Common Air. Perhaps the cause of this difference is to be fetched from hence, That a greater quantity of Water was included with Artificial Air. The Mercury mounted higher 9 or 10 digits.

May 18.

Hitherto the Onions seemed not all to be corrupted, but this day I found one of them to have contracted some corruption, which may be called a Syderation or Planet-striking, and differs from a mouldiness.

From this Experiment, made in 2 Receivers at once, we may gather, That Artificial Air doth not at all hinder Vegetation: It appears also thereby, That not onely the sensible bigness of the body, but also the quantity of the Air, is increased by Vegetation.

EX

E X P E R I M E N T VII.

*August 25.**Common Air.*

I included 6 ounces of unripe Grapes in a Receiver capable of holding 25 ounces of Water, but I did not exhaust the Air.

August 26. The Mercury ascended a little.

27. The height of the Mercury was 1 digit.

28. The height of it was $1\frac{1}{4}$.

29. The height of it was $1\frac{1}{2}$.

August 30.

The Mercury seemed to have descended rather than ascended. The colour of the Grapes was less altered here, than in the Receiver, into which Air produced out of Pears, had been immitted.

August 31.

The Receiver was broken, and I left the Grapes exposed to the free Air.

Septemb. 7.

The Grapes being left in the free Air, did still keep their green colour, and were of a taste grateful enough, though less pungent than before.

*August 25.**Fallitious Air.*

I included 2 ounces of unripe Grapes in a Receiver capable of holding 8 ounces and $\frac{1}{2}$ of Water: and having stopped it close with a Screw, I filled it further with Air, which I immitted, produced from Pears, as much as sufficed to sustain 15 digits of Mercury.

August 26.

Some Air escaped out, and therefore I immitted new Air, pro-

produced out of the same Pears, untill the Mercury staid at 17 digits above its wonted pressure.

August 27.

The Mercury was depressed below the 16 digit; and yet no Air had brake forth. Towards Evening, I found the Mercury had again ascended to 17.

$$\left. \begin{array}{l} \text{Aug. 28} \\ 29 \\ 30 \end{array} \right\} \text{The height of it was } \left\{ \begin{array}{l} 19 \\ 21 \\ 22 \end{array} \right| \begin{array}{l} \text{Aug. 31} \\ \text{Septemb. 1} \end{array} \right\} \text{The height of it was } \left\{ \begin{array}{l} 23 \frac{1}{2} \\ 24 \\ 24 \end{array} \right.$$

September 4.

The same height continued at 24. and the Grapes had all contracted a yellow colour.

Septemb. 5.

The Air broke out.

September 7.

The Air proceeding to get out by degrees, I took out the Grapes, and found them very insipid, and of an unacceptable taste.

This Experiment, made in 2 Receivers at once, doth confirm to us the efficacy of Artificial Air, to alter the colour of Fruits. 'Tis also very observable, That in this Experiment it did prejudice the preservation of the taste, and promoted the production of the Air, contrary to what had happened in the former Experiments. It would be worth the while to try, whether the same success would even with all unripe Fruits.

EXPERIMENT VIII.

August 2. 1676.

Facitious Air.

I shut up one Gilliflower in a Receiver, with Air produced from Paste made with Meal, and not mixed.

August 4.

Our Flower began to change colour and to be moist.

I

August

*The Second Continuation of**August 9.*

The Gilliflower was little altered.

August 12.

The moisture increased by little and little, but no mouldiness appeared.

*August 31.*The Gilliflower was little altered, yet it was less fresh than those which were kept *in vacuo*.*August 2.*

COMMON AIR.

I shut up one Gilliflower in a Receiver, with Common Air, not mixed.

August 4.

Our Flower was not changed.

August 9.

The Gilliflower was madid, and had almost lost all its colour.

August 12.

Now a great mouldiness covered all the Flower.

August. 2.

VACUUM.

I included two Gilliflowers *in Vacuum*; and took special care, that no humidity should be included with them.*August 4. 1676.*

One of the Gilliflowers began to appear madid

August 31. 1677.

During the whole elapsed Year, the Gilliflowers had suffered no mutation.

By this Experiment; instituted in 3 Receivers at once, it seems probable, That Factitious Air doth render the change of colour more speedy, yet it prevents mouldiness, even as *Vacuum* doth the same.

EX-

EXPERIMENT IX.

July 24.

COMMON AIR.

I put Apricocks, and some Plums, of which divers were cut in pieces, into a Receiver full of common Air, and stopped it firmly with a Screw.

July 25.

The Mercurial Gage was spoiled, and therefore I could not by any means perceive the quantity of the Air to be generated.

July 30.

The Fruits seemed not at all to be altered, saving that one of the dissected Plums had contracted something of mouldiness.

August 11.

I opened the Receiver, and found all the Fruits firm, of a good colour, and of a grateful taste.

July 24.

ARTIFICIAL AIR.

I made the same Experiment in another Receiver, with the same circumstances, save onely that into this last Receiver I intruded Air, produced from Cherries, as much as was sufficient to sustain 22 digits of Mercury.

July 25.

I found the Mercury to have descended 3 digits, it staid in 19. Toward the Evening it recovered its former height, it staid in 22.

July 26 } The height { 28
27 } of it was { 34 $\frac{1}{2}$ | July 28 } The height { 36
29 } of it was { 40
July 30. The height was 44. The Apricocks which were cut, began to moisten, and to be dissolved into water.

I 2

July

*The Second Continuation of**July 31.* The height was 51.*Aug. 1.* The height was 60.

August 2. The height was 65. Towards Evening, when I found some liquor had escaped out of the Receiver, I screwed it more straitly, but one of the iron Wires being broken, all the Air got out. Wherefore I took out the Fruits, and found them very soft, especially those whose lower parts were immersed in the Water; for the rest they were a little more firm; but all of them retained a grateful taste.

From this Experiment made in 2 Receivers, it seems to be inferrable; That Air produced from Cherries, doth promote the alteration both of colour, and also of firmness in Apricocks.

It appears also, That some part of such Air is destroyed in the beginning.

E X P E R I M E N T X.

July 30. 1676.

I put Plums, cut asunder, into 3 Receivers, of which one was full of Artificial Air, produced from Goosberries; the second was full of Common Air, the third was *Vacuous*.

August 2.

In the Artificial Air, the Plums were not changed. In the Common Air, they began to be mouldy; but in the *evacuated* Receiver, they retained their colour, but were soft.

August 5.

In the Artificial Air the Plums had contracted a red colour, humidity, and softness; In the Common Air, they seemed black and mouldy, yet retaining their firmness: In the *evacuated* Receiver, they were almost melted or dissolved:

August 7.

In the Common Air the Plums now began to soften.

August

August 8.

In the Common Air, the Plums seemed to have lost their black colour, and to have contracted a red one; even as it happened 3 days ago to the Plums in the Artificial Air.

In this Experiment, Artificial Air seems to have promoted alteration.

EXPERIMENT XI.

September 24.

I put 5 Peaches into a Receiver, with Common Air mixed with Air produced from Grapes, and I included the Grapes themselves in the same Receiver; that the Common Air might be the better saturated with the Artificial.

September 25. The height of the Mercury was 21 digits.

Sept. 26	} The height {	23	Sept. 29	} The height {	42	
27		31			30	45
28		39			Octob. 1	48

of it was

Octob. 2. The same height continued.

3. The height of it was 52 $\frac{1}{2}$.

5. The height the same; but the Peaches seemed somewhat madid.

6. The height of it was 58.

7. The height of it was the same.

8. The height of it was 61.

11. The Mercury ascended a little.

19. The height of it was 65.

25. The height of it was 61. The cold was sharp.

27. The Cold abated and the Mercury ascended.

30. The height stay'd at 61. and a little more.

Novemb. 2. The height of the Mercury was 59. 'Twas bitter cold weather.

6. The height of it was 61. The Frost broke and was dissolved.

Nov.

Nov. 7. The Mercury seemed somewhat higher.

9. The Mercury persisted in the same height.

Dec. 9. In one Months space the Mercury ascended by little and little, its height was 80 digits.

April 1. 1678.

The Mercury came to 96 digits above its wonted height, And I opened the Receiver, and whilst the Air was breaking out, the Peaches did emit many bubbles through their skin, not without violent noise, and the skin in some of them was broken; They had preserved their taste pleasant enough and the colour of their pulpe was commendable, but they had lost their firmness, as if they had been boiled; being left in the Air for 3 hours space, they were all rotten.

This Experiment proves, That Common Air doth corrupt bodies, yet it doth so much less, if it be mixed with Factitious Air.

EXPERIMENT XII.

August 4.

THE FIRST RECEIVER.

I cut 5 Pears, each of them into four parts, and I put one part of each into a Receiver full of Common Air, and stopped it close with a Screw.

August 6.

The colour of these Fruits was altered little less than of others: The Mercury ascended not at all.

August 7.

The Pears were a little altered, The Mercury was higher by a little.

August 8.

The Pears underwent no great mutation. The height of the Mercury was 4. digits.

August 9. The height of it was 4½.

Aug.

Aug. 10 } The height { 6 | Aug. 13 } The height { 16
11 } of it was { 10 | 14 } of it was { 20

The Pears began to be softned.

Aug. 15. The height of it was 21.

16. The height of it was 19. I believe the Air had got out.

17. Now I found the Air had escaped out.

18. When the Air had almost all got out since yesterday in the Evening, and I saw the Fruits to look worse than before, I took them out, and found them putrified.

August 4.

THE SECOND RECEIVER.

I took one quarter of each of the aforesaid Pears, and included it after the same manner; and afterwards I immitted Air, produced out of Cherries, till the Mercury possessed 23 digits above its wonted pressure.

August 6.

Those Fruits had altered nothing, but their colour a little.

August 7.

The Pears, almost all, seemed rotten. The Mercury persisted in the same height.

August 8.

The Pears not altered much more. Something hindered, that I could not see the Mercury.

August 10.

The Pears wax more and more soft. Now looking upon the height of the Mercury, it was 40 digits more than its wonted height.

Aug. 11 } The height { 51 | Aug. 14 } The height { 67
13 } of it was { 61 | 15 } of it was { 73

Aug. 16. The Mercury descended; yet I know assuredly that nothing had got out.

Aug.

August 17.

The Mercury exceeded not 67 digits in height, yet the Air could by no means escape out.

August 18.

The Mercury persisted at the same height, but I suffered the Air to break forth; it affected my Nostrils with a sharp odour: moreover the taste of the Fruits seemed very acid, and their pulpe exceeding soft.

August 4. 1677.

THE THIRD RECEIVER.

I put a quarter of each of the foresaid Pears into a Receiver, not exactly shut.

August 6.

The Pears seemed to change their colour.

August 7.

One of our pieces of Pears began to lose its firmness: but in the Artificial Air another piece of the same Pear did yesterday seem wholly rotten.

August 8.

One piece was mouldy, the rest were soft.

August 9.

The Pears grew more and more rotten.

August 11.

The Pears were wholly mucid and rotten.

This Receiver compared with the first, shews, That Corruption doth not begin in *Free Air* sooner than in *included Air*; but when it is begun, it is much more, yea, and more speedily increased, *viz.* because the included Air might be satiated.

August 4. 1677.

THE FOURTH RECEIVER.

I included one quarter of each of the said Pears *in Vacuo.*

August

<i>August 6.</i> The height of the Mercury was 5.	
<i>August</i>	<i>August</i>
7	13
8	14
9	15
10	17
11	20.

The height of it was { 8, 10, 12, 14, 16 } The height of it was { 20, 23, 25, 28 }

had undergone no alteration, but this day they began to be soft : The Mercury ascended not.

August 26. Neither the Pears, nor the height of the Mercury were altered at all.

This production of the Air seems very regular.

By this Experiment, made in 4 Receivers at once, we find the aptitude of Artificial Air for the softning of Fruits.

And that the production of Air was here promoted by Artificial Air, is very probable ; yet it had succeeded otherwise with Apricocks, *Artic.II. Exper.VII.*

EXPERIMENT XIII.

August 21. 1677.

THE FIRST RECEIVER.

I divided 6 Apricocks, each into 4 parts, and I put one piece of each into a Receiver full of Common Air, and stopped it firmly with a Screw.

Aug. 22.

The Apricocks seemed riper this day than yesterday ; but no Air was produced by them.

August 23.

One piece, contiguous to the Water, began to be mouldy, the rest inclined to putrifaction : the Mercury seemed to have ascended a little.

Aug. 24.

A piece next the Water, was covered with a great deal of
K mouldi-

mouldiness, another piece, more remote from the Water, was somewhat mouldy also; but all were rotten.

Aug. 25.

The Fruits contracted no more mouldiness; but the putrefaction more and more increased. The height of the Mercury was 7 digits.

Aug. 26. The height of the Mercury was 15. digits.

28. The height of it was 30.

29. The same height continued.

30. The height of it was 33. The Fruits were almost all dissolved.

31. The height of it was 38.

Septemb. 1. The height of the Mercury was the same.

2. The same height still.

3. The Mercury ascended a little.

Septemb. 4	} The height	{ 41	Sept. 7	} The height	{ 45
5					

Septemb. 9. The same height continued.

Sept. 22. Little or no change was made in the height of the Mercury; but the Fruits were almost melted into water.

Octob. 1.

When the Mercury continued in the same height, and the Fruits were almost all vanished, I opened the Receiver, and found the Apricocks very much impaired, and soft, yet they had retained a taste, not ungrateful, but subacid.

August 21. 1677.

THE SECOND RECEIVER.

I covered one quarter of each of the foresaid Fruits, the Receiver not being fortified against external Air.

Aug. 22.

The Apricocks were *flaccid* or quailed, as if they had been dry or withered.

Aug.

Aug. 23.

Many of our Fruits appeared rotten and mouldy.

Aug. 24.

The Apricocks were wholly infected with putrefaction and mouldiness.

August 21.

THE THIRD RECEIVER.

I included firmly by the help of a Screw, one quarter of each of the foresaid Fruits, in an unexhausted Receiver; to which I after added Air produced from Pears, as much as sufficed to sustain 20 digits of Mercury.

Aug. 22.

The Mercury ascended not at all; but the Fruits seemed to have acquired a greater degree of maturity than those which are included in Common Air.

Aug. 23.

These Fruits seemed less altered than they which were in Common Air.

Aug. 24.

The Fruits were not altered.

Aug. 25.

The Fruits did begin to produce Air, but I could not discern the quantity.

Aug. 26.

Little alteration in the Fruits.

Aug. 28.

The Apricocks began to moisten, yet they were far less altered than those which remain in Common Air.

Aug. 30.

The Mercury did this day emerge above the bodies by which it was hid. Its height above the wonted pressure, was 30 digits.

Aug. 31. The height of the Mercury was 40 digits.

Sept. 1. The height of it was the same.

2. The same height continues.

3. The height thereof 45.

8. The height was little changed.

9. The height was 40. and yet no Air got out.

11. The height was 38.

12. The Mercury continued to descend.

13. The height of it was 33.

Sept. 14. The Mercury was so depressed, that it appeared no more.

Sept. 22. The Mercury did emerge again, its height was 33. The Fruits were covered with a kind of *mucor* or Finew.

Octob. 1.

When the height of the Mercury, nor the Apricocks, were any more altered, and the Finew vanished away, I opened the Receiver, and found the Apricocks not impaired, but of a colour laudable enough, but their pulp was spongy and soft, and of a subacid taste.

August 21.

THE FOURTH RECEIVER.

I took a quarter of each of the aforesaid Fruits, and shut them up firmly with a Screw in an unexhausted Receiver, into which afterwards I intruded Air, till the Mercury came to 90 digits above its accustomed pressure.

Aug. 22.

Our Receiver broke into an hundred-pieces by the force of the Air compressed within it: whereupon I put the Fruits into another Receiver, and added onely such a quantity of Air as was able to sustain 60 digits of Mercury.

Aug. 25.

The Apricocks had contracted no mouldiness, I added new Air.

Aug.

August 26.

The Apricocks were wholly infected with mouldiness, and rottenness,

This Receiver, if compared with the former, doth shew, That the quantity of corruption, doth depend on the quantity of the Air.

By this Experiment made in 4 Receivers at once, we have a confirmation, That in Factitious Air alteration is made quicker; but in tract of time, the corruption is far greater in Common Air.

ARTICLE IV.

The Effects of Compressed Air, are different from the Effects of Common Air.

EXPERIMENT I.

March 21. 1677.

I Put 2 Onions into a Receiver, which was to be stopped close with a Screw, and I intruded so much Common Air thereinto, that raised the Mercury 60 digits above its wonted pressure.

March 28.

My Onions took root as well as other Onions which I had included in Common Air at the same time.

April 28.

The Onions included in Common Air 8 days ago, were covered with mouldiness, though in the beginning they had put forth roots numerous enough: The Onions in the other Receiver began to contract corruption at the ends of their roots, but the compressed Air 10 days before had found a gradual passage.

passage out, and now was almost all escaped. And therefore I put in new Air, till the Mercury had attained to the height of 60 digits above its accustomed pressure.

April 29.

The Onions in the compressed Air, were all over covered with mouldiness.

From this Experiment it seems to follow, That a little compression doth not prejudice those bodies which are to be expanded by vegetation.

Moreover the new Air, which was intruded, seems to have promoted the mouldiness, though in the beginning it is probable that the compression of the Air did retard both the mouldiness, and also the corruption.

EXPERIMENT. II.

May 9.

I put 2 equal quantities of Tulips and Lark-spurs into 2 Receivers of an equal bigness, and stopped them up firmly with Screws: I left one of them with Common Air onely, but I compressed the other with the intrusion of new Air, till the Mercury did exceed its wonted height by 70 digits.

May 11.

Two Tulips in the Common Air contracted mouldiness, but all things remained unaltered in the compressed Air.

May 12.

A third Tulip, in the Common Air, began to be finewed; but there was no such thing in the compressed Air.

May 14.

This day I perceived one Tulip in the compressed Air to be infected with some *mucor* or finew, but those which remained in the Common Air, were all very mucid, and also one of the Lark-spurs in the Common Air, had contracted a *mucor*.

May

May 17.

Three of the Tulips in the compressed Air had indeed contracted a Finew, but not half so much as Tulips in the Common Air were covered with. And moreover 2 of the Lark-spurs in the Common Air appeared finewed also; but those shut up in compressed Air, were preserved fresh, and wholly free from mouldiness or finew.

May 21.

The Flowers in the Common Air were all rotten and putrified; but the other in the Compressed Air, received no further alteration: and besides, the Tulips, which had contracted some finew, seemed rather to lose *that*, than to acquire *new*.

May 30.

When the Flowers in the common Air, being wholly putrid, were dissolved into water, I took them out, and kept the liquor in the Vessel to try whether any Insects would breed therein. In the compressed Air the Flowers suffered no more sensible alteration; and therefore I took them out, and found them madid, and infected with a subacid odour.

By this Experiment it seems plain, That compressed Air doth hinder putrefaction and mouldiness in some plants.

EXPERIMENT III.

May 21. 1677.

I cut an Orange into two equal parts, and one of the halves I stopped up in a Receiver with Air so compressed, that it would sustain 100 digits of Mercury above its wonted pressure; but I left the other half in another Receiver, well shut, onely with common Air.

May 25.

Each half of the Orange had contracted mouldiness, but that which was in the common Air was much more mucid than the other.

May

May 26.

This day I perceived that the compressed Air had almost all got out, and therefore I put in new.

May 30.

Every day I perceived some Air had got forth, and therefore I made a dayly supply by adding new. And it came to pass that the Orange by receiving new air, so often admitted, had contracted a *mucor* notwithstanding the compression much more than the other piece of Orange that was always left in the same air without pressure.

June 1.

I took out the two half Oranges, and that which remained in the compressed air, seemed to have contracted a corruption at least three times greater than that which had continued in the common air.

By this Experiment, The aptitude of compressed air, to retard corruption, is confirmed; yet in progress of time 'tis very probable, that the quantity of corruption doth depend upon the quantity of the air. See *Exper. 1.*

E X P E R I M E N T I V.

May 31. 1677.

I included two equal quantities of Roses in 2 Receivers, which I stopped by the help of Screws, into one of which I intruded as much air as would suffice to sustain 90 digits of Mercury, besides its accustomed pressure; but I left the other onely with common air.

June 11.

The Roses in the common air were free from mouldiness, onely they seemed to have lost something of their colour; but those which were shut up in the compressed air had almost all contracted a yellow colour, as if they had withered in the open air, and yet they were not mucid or finewed.

June

June 18.

This last Week the Flowers in the common air admitted not the least change; but those in the compressed air grew more and more yellow. I opened both Receivers, and found the Roses to have kept their smell, yet it was somewhat altered, neither of them were dry nor withered: I kept them apart in the open air, and found that the Roses, taken out from the compressed air, were not so soon altered by the contact of new air, as those which had remained in the air not compressed.

From this Experiment it seems to follow, That compressed air is sometimes fitter for the alteration of colour than common air. And perhaps it may not be unworthy of our notice, that Roses so included, contract not a mouldiness, but only a yellow colour; but in Tulips and Larkspurs the matter succeeded otherwise. See *Exper. II.*

EXPERIMENT V.

June 1. 1677.

I put the 2 halves of the same Orange in 2 Receivers; In the one I increased the quantity of air till it sustained the Mercury 100 digits above its wonted height; but I left the other un-compressed, only exactly shut.

June 6.

Each half of the Orange was infected with mouldiness, especially that, whose ambient air was compressed. But note that new air was every day to be supplied thereunto; for the compressed air in 24 hours space had almost all got out. But in *Exper. III.* it had remained very well shut in for 6 whole days.

June 11.

The Orange in the common air contracted no more mouldiness; but in the compressed air, the *mucor* or mouldiness was more and more increased.

L June

June 18.

Finding the mouldiness of the Orange in the common air to be lessened rather than increased, I took it out; and perceiving further, That in compressed air the Orange was not more mucid, after I had ceased to intrude new air; I was willing to try, whether the new air did suppeditate new strength to the Orange to exert and thrust out its mouldiness; therefore I made the Mercury in the Gage, by reason of the air I intruded, to exceed its wonted height 80 digits.

June 20.

Two days after I had intruded new air into the Receiver, the mouldiness of the Orange appeared to be manifestly augmented.

From this Experiment we may gather, That the quantity of the mouldiness doth depend on the quantity of the air.

E X P E R I M E N T VI.

June 17. 1677.

I put 2 Shrew-Mice into 2 Receivers, of equal bigness, and stopped them up carefully; In one of them I left onely common air; into the other, I intruded air, till the Mercury was higher than its wonted pressure 30 digits: But the Mouse in the common air was included about 5 and 52', 6' after the other.

The Mouse in the compressed air seemed to lose his strength much sooner than the other, the motion of his breast being less frequent. Yet notwithstanding about 6 and 18', the Mouse in the common air, which seemed the stronger, fell into convulsive fits and died; but the Mouse in the compressed air, seemed then, and some time after, to be as well, as it was an hour and half before.

About 11 of the Clock, the mouse in the compressed air did as yet breath; but about 4 in the morning he was found dead
in

in the same posture, wherein he was 7 hours before ; whence we may conjecture, that he was free from convulsive fits.

I must not here omit to relate, that the Mouse in the common air had consumed something of that air, so that the Mercury stood at 29 digits, which, when the Receiver was opened, presently ascended to 30.

From this Experiment we learn, That compressed air seems fitter than common air, for the prolongation of Life, seeing the one Mouse lived 24' and no more, but the other lived about 15 turns longer, though only a double quantity of Air was included in his Receiver.

EXPERIMENT VII.

June 13. 1677.

I put 4 Flies into a Receiver, into which I afterwards intruded air, till the Mercury did occupy 60 digits above its wonted height ; and at the same time I included 3 other Flies in another Receiver, with common air not compressed.

July 14.

This day in the morning all the Flies were well. In the afternoon I found 2 of them dead in the compressed air, but in the common air they were all alive. About 5 of the clock one of the Flies in the compressed air was alive and three in the common air.

June 15.

This morning I found all the Flies in the common air dead ; but that single one which remained alive in the compressed air, seemed still to be very well, and being taken out of the Receiver, flew speedily away.

From this Experiment it seems to follow, That Flies are not very sensible of the compression of the air ; and that they die more for hunger than for default of air : for the Flie which was so long well, fed upon the carcases of those which were

L 2.

dead,

dead, so that she seemed to be affected with no distemper. Yet I iterated the Experiment. See *Exper. VIII.*

EXPERIMENT VIII.

June 15.

I repeated the former Experiment, only including 4 Flies in each Receiver, and compressing the air somewhat more.

June 16.

This morning I found 2 of the Flies in the common air dead, and but one in the compressed air.

About 2 in the afternoon the 4 Flies in the common air seemed to be dead, but in the compressed air, the 3 were alive.

June 17.

All the Flies died, except one in the compressed air.

From this, and the former Experiment, a man may conjecture, That the compression of the air is of small consequence to Flies; and indeed they are not prejudiced by the rarefaction of the air, but with great difficulty, unless there be almost a complet *vacuum*.

EXPERIMENT IX.

June 18.

I included 2 Frogs in 2 Receivers, and stopped them by the help of Screws; the one only with common air, the other with air compressed to sustain 70 digits of Mercury.

June 19.

Both the Frogs were alive; and the height of the Mercury in both Receivers remained the same.

June 20.

Neither of the Frogs were dead, and they seemed to me rather to diminish than increase the air, but the difference was so small, that I dare not be positive therein.

JUNE

June 21.

In the morning both the Frogs were alive; but towards evening the Frog in the common air was found dead.

June 22.

At evening the Frog in the compressed air was alive.

June 23.

In the morning I found the Frog dead.

It must be found out by iterated Experiments, whether the greater length of life was to be ascribed to the compression of the air, or to the disposition of the Frogs.

EXPERIMENT X.

June 18. 1677.

I shut 2 half parts of the same Orange in 2 Receivers, and stopped them by the help of Screws; the one with common air, the other with air compressed to sustain 90-digits of Mercury.

June 22.

This morning I found the Orange in the common air, to be infected with mouldiness, but the other was sound.

At 3 of the clock in the afternoon, the Orange in the compressed air seemed also to have contracted some *mucor*.

June 23.

I found the Orange in the common air far more mucid than the other.

June 24.

The Orange in the common air did not increase his mouldiness, but the other was covered all over with it.

June 28.

The mouldiness produced in the common air was now wholly vanished; In the other Receiver, I saw no further alteration in the Fruit.

June 30.

June 30.

Perceiving that the Fruits persisted in the same state, I took them out. The half Orange, which was kept in common air, seemed half rotten; but the other besides its finew, appeared wholly putrified.

By this Experiment we have a confirmation, That the quantity of the mouldiness or finew doth depend on the quantity of the air.

It seems also worthy of observation, That the mouldiness, or hoariness did appear a little later in the *compressed* air than in the *common*, though afterwards it increased much more.

EXPERIMENT XI.

June 29. 1677.

I included Roses in 2 Receivers, stop'd by the help of Screws; I left one with common air onely, but I filled the other with so much air intruded by force, that the Mercury ascended to 90 digits above its wonted pressure.

July 14.

Four or five days ago I found the Roses in the compressed air to wither and to degenerate into a yellow colour. There was not the least alteration in the other Receiver.

July 17.

When I perceived that this present Experiment proceeded after the same manner, as That mentioned *p.* 72. I took out the Roses. Those kept in the compressed air, were very much corrupted, and of a very ungrateful smell; but the others were little altered; and their smell not unpleasant.

Hence we have a further confirmation, That the quantity of corruption doth depend on the quantity of the air.

EX-

EXPERIMENT XII.

July 4.

I cut a Limon afunder, and put both halves into two Receivers, to be stopped by the help of Screws: The one I left with common air onely, but the other I filled with so much compressed air, that it sustained 90 digits of Mercury above its wonted pressure.

July 7.

This day both parts of the Limon seemed to grow mouldy at the same time.

July 17.

The part of the Limon in the compressed air, had contracted much more of hoar or finew, than the other: And perceiving no further alteration in them, I took them out, and found the Limon in the compressed air far more putrid than the other.

By this Experiment, it is confirmed, That the quantity of corruption doth depend on the quantity of the air.

It seems also, That a triple compression of the air, in respect of a Limon, is too weak sensibly to retard the production of mouldiness or finew.

EXPERIMENT XIII.

July 18. 1677.

I included 2 parcels of Gilliflowers, equal in number, in 2 equal Receivers, and stopped them close with Screws. I filled the one with compressed air, till it sustained 100 digits of Mercury above the wonted pressure; but the other was left with common air alone.

July 23.

In the compressed air, the Gilliflowers were bedew'd with some hoariness or mould; the others appeared onely moist:
but

But the Mercury exceeded its wonted height onely 70 digits, because some of the air had got forth.

July 25.

In the compressed air, the Gilliflowers proceeded to be much more corrupted than the others: They had wholly lost their colour.

July 26.

In the compressed air, the Gilliflowers were wholly putrified, and covered with an hoary finew; the others were moist onely in some places.

August 1.

Perceiving no farther alteration in the Gilliflowers, I took them out of their Receivers; those which were kept in compressed air were rotten, and did stinke; but the other kept their colour, and their smell was not offensive, but they were moist.

This Experiment confirms, That the quantity of the air doth increase corruption.

We may also observe, That the mouldiness or hoariness is not produced, but in compressed air; neither is it probable that this happened by chance, seeing in each Receiver there were 4 Gilliflowers included, or three at least.

EXPERIMENT XIV.

July 21. 1677.

I included a Shrew-Mouse in a Recipient, with common air, and shut it in firmly with a Screw, to trie whether he would produce or consume air.

After 2 hours the Mouse died, and some air was consumed, but a less quantity than in the Experiment mentioned *p.* 74.

July 24.

Hitherto I found no change in the height of the Mercury. Towards evening it seemed a little higher.

July

July 25.

This day in the morning much air was produced *de novo*.

July 26.

The quantity of the produced air increased more and more.

By this Experiment we have a confirmation, That *living* Animals do consume air, but *dead* ones produce new.

EXPERIMENT XV.

August 31.

COMPRESSED AIR.

I put Pears into a Receiver, whereto, after it was well stopped, I added as much Air, as sufficed to sustain 30 digits of Mercury above the wonted preffure.

September 1.

The Mercury wasdepressed, as it happened fol.37.

Sept. 2.

The height of the Mercury decreased: it exceeded not 25 digits.

Sept. 3.

This day the Mercury proceeded one digit higher; it staid in 26.

Sept. 4.

The height thereof was 28.

Sept. 8.

Because the Receiver did afford some efflux to the air, I therefore put in new: And this day, opening the Receiver, to compare the taste of these Fruits with the taste of the others, I found that 5 of the Pears had lost their firmness, but 2 had retained it.

M

August

August 31.

COMMON AIR.

I included Pears of the same kind in another Receiver, with common air onely, not compressed.

September 1.

The Mercury was a little depressed, as if it had been in compressed air: The cause whereof I judge attributable onely to the Cold.

Sept. 2. The Mercury was not changed.

Sept. 3.

The height of the Mercury was one digit above the wonted pressure.

Sept. 4	} The height	{ 4		Sept. 6	} The height	{ 6 $\frac{1}{2}$
5				of it was		

September 8.

The height of the Mercury was 20. The Pears being taken out of the Receiver, had preserved their taste much better than those which were included *in vacuo*. They also retained their firmness.

August 31.

VACUUM.

I included Pears of the same sort *in vacuo*, but some external air brake in, and the height of the Mercury was 1 digit.

Sept. 1	} The height	{ 4		Sept. 5	} The height	{ 19				
2				8			6	23		
3				of it was			{ 12	7	of it was	{ 27
4				16			8	30		

The Pears, being taken out, had kept their firmness, but had lost much of their taste.

From this Experiment, made in 3 Receivers at once, it seems to follow, That in a greater compression, a less quantity of air is produced.

EX-

EXPERIMENT XVI.

December 7.

I shut up a small Bird in a Receiver, capable of holding 20 ounces of Water. The Bird began to be ill, before I had *set* the Screw; but, after I had intruded so much air, as could sustain 30 digits of Mercury above its wonted height, she seemed to recover again; but in some space of time after, she began again to be sick, and therefore I intruded air the second time, till the Mercury staid in 45 digits above its wonted height, and then the Bird was again restored to health, but a little time after she began to gasp again; then opening the Receiver, after she had staid in it 28 minutes, she got out, and was very well.

EXPERIMENT XVII.

January 20. 1678.

I put a Shrew-Mouse into the Receiver of my Wind-Gun, whose elliptick aperture was scituate in its upper part, the Figure of it is set down *p. 16, 17*. Then as quick as I could, I so far condensed the air there, till it was reduced to the twentieth part of its space, or thereabouts; and then I presently discharged that Air, and the elliptick hole being opened, I suspected that the Mouse had been onely a little convulsive; but when he was taken out, there were no signs of life in him. And therefore 'tis left to enquiry, Whether the cause of his death were to be ascribed to the Narrowness of the Receiver, or to the Compression of the Air?

Wherefore I put another Mouse into the same Receiver, and the air being reduced to a third or fourth part of its space, I opened the Receiver, but not so carefully as I had done in the former Experiment; yet the Mouse, taken out therefrom, was found to be very well.

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I afterward repeated the same Experiment, the air being about 7 or 8 times condensed, and the Mouse seemed to suffer no inconvenience thereby.

I tried the same Experiment again, in Air compressed 7 times, and left the Mouse included for 24 minutes; which time being elapsed, I discharged the Air, and the hole being opened, I perceived the Mouse to fetch many deep groans, as it were; yet, being taken out, he could not recover his health again.

By these Experiments it is manifest, That a great compression of Air is noxious, yea mortiferous to Animals.

EXPERIMENT XVIII.

January 28. 1678.

I put a Shrew-Mouse into a Glass, to whose neck I tied a bladder stopping the orifice. These things being thus prepared, I put them into a Receiver for the compressing of the Air. A little time after, when the Mouse began to be sick, I compressed the Air, and the bladder was straitned, and so the Mouse was found in compressed Air, though no new Air could penetrate to him: Then he seemed to be much better, and his heart did not pant so often; and opening the Receiver, in a short time, he was as well as ever.

I iterated the same Experiment, and the Mouse was left there so long, that he could hardly breath, whilest I began to compress the Air; and the compression seemed again to abate his respiration; the Receiver, being opened, and so the Mouse exposed to the Air, could not breath much more freely; but if I blew the Air on him by Bellows, he seemed to be something relieved; but being again committed to the compressed Air, he breathed less frequently, and at last died.

March, 25.

Because in the former Experiment it was not clearly manifest, whether the Air did enter through the ligature of the bladder,

der, I used the Instrument described *p.* 15. And when I perceived that the Mouse was sick, and breathed seldom, I intruded Water into the Receiver, so that the Air was reduced to the half of its space, and then the Mouse breathed more rarely; but if, extracting the Water, I left the whole space entire for the Air, his respiration seemed more vivid, and the Air being thus many times contracted and dilated, the sick Mouse seemed to me to breath more lively in the common Air, than in the compressed. Whence I conjectured, That the Air is to Animals, like Food, the quantity whereof ought to bear some proportion with their strength: and that I might more certainly know it, I put the same Mouse into my pneumatick Engine, and rarified the Air, so that it possessed more than double the space it was wont; whilst the Air was rarefying, presently the Mouse began to be better; yet a little while after he seemed to be sick, and when the Air was restored, it brought no sensible commodity or inconvenience to the Mouse. I thus repeated the rarefaction three times, and the same success followed; but at last the Mouse died.

ARTICLE V.

The Effects of Artificial Air upon Animals.

EXPERIMENT. I.

May 5. 1677.

I Put a Bee, with Vinegar distilled, and pulverized Coral, into an emptied Recipient, and the Air being wholly exhausted, I ordered the matter so, that the Coral fell down into the Glass of Vinegar: But the Air, produced from thence, did not restore

restore any power of motion to the Bee; but when she was exposed to the open Air, in a little time after she began to move her self.

Hence a suspicion doth arise, That Artificial Air is unfit for the life of Animals.

EXPERIMENT. II.

August 12. 1676.

I put 2 Flies into a Receiver, and exhausting the Common Air, I substituted Air, produced from Goosberries, in its place, as much as could sustain 26 digits of Mercury.

Afterwards I put 2 other Flies also *in vacuo*; but with this difference, that I restored common Air to these latter Flies, only in that quantity, as could sustain 23 digits of Mercury.

Within a quarter of an hour; these latter Flies, upon the restitution of the Air, recovered that power of motion which they had lost *in vacuo*, and did flie in the rarefied Air; but the former lay without any motion, though they had received a greater quantity of Air.

August 13.

The Flies in the artificial Air, seemed still dead; but the others were lusty.

The Flies taken out of the artificial Air, and exposed to the common air, remained so all this whole day, and yet did not recover any life.

August. 18.

I renewed the same Experiment, with the same success, though I had restored a greater quantity of artificial air.

Hence we have an high confirmation, That artificial air is noxious to the life of Animals.

EX.

E X P E R I M E N T I I I .

June 22. 1677.

I put Paste into 3 Receivers, out of which I afterwards exhausted the Air.

June 23.

When my 3 Receivers did this day regurgitate with Air produced from the Paste, I kindled a perfumed Cone, and thus kindled, I put it into one of my Receivers, which being presently stopped, the Fire, within one minute of time, went out. Then by blowing, I expelled the artificial Air from the Receiver, and put in fire to it, as before; and then it burned bright for a pretty long time, though I had shut the Receiver as speedily, and as accurately as before.

I tried another Experiment, after the same manner, with a Fly, and in the artificial Air she was presently dead as it were, but afterward, being exposed to the Sun, she in a short time grew well again. Then I blowed in common Air into the Receiver, which being done, the Fly included as before, suffered no inconvenience thereby.

I iterated the self-same Experiment with the same Fly in our third Receiver, being filled with Artificial Air, and the same success followed, save onely that this Fly, when it was taken out from the artificial Air, could not be restored to health, but in a longer time, *viz.* because she was left there longer.

By these Experiments it appears, That factitious Air is prejudicial to Fire, as well as to the life of Animals.

E X P E R I M E N T I V .

June 25. 1677.

I put Paste into 4 Receivers, and exhausting the Air wholly
from

from two of them, I pump'd out onely half the Air from the other two.

June 26.

I found the 2 Receivers which I had left half full with common Air, to be quite filled with Air newly produced; neither dare I assert, whether they had for some time regurgitated or no, so that the quantity of common Air was much diminished. However the matter was, I put 2 Flies at once into one of the Receivers, after the manner before described; and they, as soon as they touched the bottom of the Receiver, in a very little while after remained without motion. I put a third Fly into the Receiver, after the same manner, and found she lived a little longer there than the former. A fourth Fly, being thrust in, maintained her life longest of all, yet at last, suffering some convulsion, she lay unmoved and resupine. All the Flies, after some stay in the artificial Air, being taken out from thence, and exposed to the common, grew well in a short time.

I made the same Experiments in another Receiver half full of artificial Air, and in a manner with the same success; but the Flies, in that Receiver, to which onely common Air was blown in, recovered the power of motion and their strength in a short time.

June 27.

I found one of the Receivers, which was wholly evacuated of common Air, to be full of artificial Air; but it being casually thrown down upon the ground, ingress was thereby afforded to the external Air: yet I put a Frog into it, which seemed not to be very sick therein.

June 30.

My fourth Receiver, by the power of the produced Air, seemed at length forced away from his Cover. I put a Frog into it, in manner aforesaid, and she fell into high Convulsions for five minutes space, and then lay without motion. After four minutes were elapsed, I opened the Receiver, and taking
out

out the Frog, for 46 minutes she remained without motion; but afterwards in four or five minutes more she grew very well.

By these Experiments, it is evident, That artificial Air is very hurtful to the life of Animals; but if it be mixed with common Air, it doth not so readily produce its effects.

EXPERIMENT V.

June 28. 1677.

I put Paste into 4 Receivers, 3 of which I caused to be wholly exhausted of common Air, but the fourth was left half full of Air.

June 29.

One of the Receivers which were wholly exhausted, was found full of Air newly produced; and a Frog being put into it for 4 or 5 minutes, had strong Convulsive fits; then for one minute it lay still without motion, whereupon I took the Frog out, and in 5 minutes she began to move, and a while after became well again.

I took another Receiver, filled with artificial Air, and putting a Frog into it, 7 minutes were elapsed before she ceased to be convulsive. And afterward, when she had lain 1 minute there without motion, I opened the Receiver, and taking out the Frog, found that she began to struggle and move, yet I judged those motions to be the reliicks of her Convulsions; for after that she remained unmoved for a whole half hour and more; yet at last she grew well again.

As for that Receiver, from which I had exhausted onely half of the Air, it had so long regurgitated with produced Air, that it is very credible, much common Air had got out together with it. A Frog being cast into it, seemed to be vehemently moved, and convulsive for 10 minutes, as the rest did, and then she seemed quite dead; but after a full minute was elapsed, I

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opened the Receiver, and the Frog, being exposed to the open Air, within a quarter of an hour began to recover motion again.

I put a Frog into a Recipient, full of common Air, to trie, whether, the Paste being now taken out, the Frog would continue her life any longer time there ?

July 1.

In the afternoon, I found the Frog dead, in the morning she was alive and breathed, so that she lived about 48 hours.

June 30.

I cast a Frog into my fourth Receiver, which was wholly filled with artificial Air; for 7 minutes and an half she was vehemently convulsive, and at last died; then after 2 minutes, she was taken out of the Recipient, and yet recovered no motion at all.

July 1.

Perceiving the Frog to remain in the same posture, I threw her away.

We have a confirmation by these Experiments, That artificial Air is so much the more hurtful to Animals, by how much the freer it is from common Air.

EXPERIMENT VI.

June 30.

I included Paste in two Receivers, and then I exhausted the Air.

July 4.

I would have put a Shrew-Mouse, being taken by the tail, into one of my Receivers, filled with artificial Air, but the little Vermine, with his fore-feet, did so catch at the edges of the Receiver, that he could not then be thrust into it; and by this means the Receiver, being for a while open, afforded ingress to the external Air; yet I shut it again, till I had bound the legs
of

of the Mouse, and then he was easily put in, and there suffered vehement Convulsions, and after the elapse of one minute, died, I presently took him out, and exposed him to the common Air; but his life being wholly gone, no power of motion could be recovered.

Then I took the other Receiver, and putting a Snail into it, did with some wonder observe, that he continued to be moved very strongly for a whole quarter of an hour; but afterwards his motion was slower, untill about another quarter of an hour being elapsed, he lay still, as if he were dead; but then being taken out of the Receiver, and exposed to the Air, in a short time he grew well.

I put Flies into the same Receiver; but now it had admitted too great a quantity of external Air, for the Flies suffered no prejudice.

By this Experiment we gather, That artificial Air doth kill Animals by some venomous quality, and not onely by the defect of common Air; for the Snails lived a longer time *in vacuo*. See *Artic. VI. Exper. III.*

EXPERIMENT VII.

July 5. 1677.

I took a Receiver, filled with Air produced from Cherries, and then transmitted that Air out of *that* into another Receiver, full of common Air, in which a Frog was kept: Matters were so ordered, that the Water gave place onely to the artificial Air entering in, and the Water it self flowed out: And thus the Frog, being included in pure artificial Air, for a quarter of an hour and more suffered Convulsions, and at last lay still without motion: yet being after taken forth, and exposed to the open Air, she grew quickly well.

It seems probable by this Experiment, That Air produced from Cherries, is less hurtful to Frogs than *that* produced from Paste. See *Exper. V.*

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EXPERIMENT VIII.

July 9. 1677.

I put Goosberries into three empty Receivers.

July 20.

I found one of my Recipients severed from his Cover by the force of the produced Air; I cast a Flie into it, which died in one *punctum* of time; a second Flie being likewise cast into the Receiver, presently also died: a third Flie put into the same Receiver, seemed a little while to be convulsive there; but less than a fourth Flie, which I included there, which yet before one quarter of a minute was elapsed, lay unmoved; afterward I dispelled the artificial Air out of the Receiver, by blowing, and in a little time the Flies grew well.

July 24.

I took another Receiver, filled with Air produced from Goosberries, and putting a Shrew-Mouse into it, found that he died there in the space of one half minute.

From this Experiment, it seems inferrable, That Air produced from Fruits, is less hurtful to Animals than Air produced from Minerals. For the 20 day of July I tried, that a Mouse did not live above a quarter of a minute in Air produced out of Gunpowder.

EXPERIMENT IX.

July 5. 1677.

I included Paste in 4 Receivers, having the Air exhausted from them.

July 6.

One of those Receivers, being filled with factitious Air, was forced from its Cover, which I again stopped, yet not so suddenly, but some *common* air might mix with the *artificial*: yet I put

put a Shrew-Mouse into it, who was presently highly convulsive, and after one minute and an half remained unmoved; and, being presently taken out, he seemed to make some convulsive motions, but died notwithstanding.

July 7.

I took a second Receiver, filled with artificial Air, and having put a little Bird into it, I suddenly stopped it; she presently fell into convulsive motions, and within a quarter of a minute, or a little more, died; I took her out, but it was too late, for she never stirred more.

I blew out the artificial Air from the Receiver, and then, another Bird of the same kind, being put into it, was very well, yet she staid there 4 minutes.

July 9.

I took a third Receiver full of artificial Air, and put that Bird into it, which in the former Experiment had continued well, and yet seemed to be lively and sound; before she had been there a full quarter of a minute, she lay without motion, and being presently taken out, there appeared no sign of life in her.

In the afternoon I put an Adder into my fourth Receiver, and within 2 minutes he began to be ill, and to gape and pant; yet he was not wholly deprived of motion till after 24 minutes. Then after 6 minutes more, which made up half an hour, I took the Adder out of the Receiver, motionless as he was, and exposed him to the free Air, yet he did not Recover life.

July 10.

The Adder remained in the same state, and gave no hope of reviviscence.

EXPERIMENT X.

July 12. 1678.

I put a Bird into a Receiver full of Air produced out of Raisins

sins of the Sun; she died in $\frac{1}{4}$ of a minute, and though I took her out presently, yet she never stirred more.

July 18.

I likewise put a Shrew-Mouse into a Receiver full of Air produced from Raisins of the Sun; but a thred left on the edge of the Receiver, hindered me from stopping it close; yet the Mouse presently began to be very ill, and after 2 minutes he lay, as it were without any motion; yet being taken out, in 2 or 3 minutes time he was well again.

EXPERIMENT XI.

October 1. 1678.

About 10 of the Clock in the morning, I included a Shrew-Mouse with common Air, in a Receiver, fortified against the external Air; about 11 the Mouse was brought to such straits, that he could hardly breath: I threw in another strong and lusty Mouse into the same Receiver, and presently put on the stopple again: But because the first Mouse had consumed some of the Air, it came to pass that the external Air was forcibly impelled into the Receiver, and so was able to dispel a great part of the Air stagnant there; and indeed, when this was done, the first Mouse seemed to be much better, neither did it die much sooner than the other, but both of them died about noon. About 4 in the afternoon, I thrust a fresh strong Mouse into the same Receiver, and lest the external Air might again expel the included Air, I put him in very slowly and liesurely; The issue was, that this third Mouse lived not 3 minutes entire.

Whence we may conjecture, That that portion of Air which hath once served the respiration of Animals as much as it could, is no longer useful for the respiration of another Animal, at least of the same kind.

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EXPERIMENT XII.

April 28.

This day in the morning I put so great a quantity of Paste into an empty Receiver, that in the afternoon I found the Receiver full of factitious air; whereupon I thrust down a Snail into it, which presently frothed very much, and did very often expand and again contract it self; but at length after 4 minutes were elapsed, he ceased to move at all, yet I took him not forth, till he had staid in the Receiver an whole quarter of an hour, and then, being extracted, he seemed as if he had been quite dead; for though he were pricked with a pin, yet he discovered no sign of life; yet after another quarter of an hour, being also pricked with a pin, he made a little motion.

I blew out the factitious air from my Receiver, and then thrusting in another Snail after the same manner, as I did the former, he was very well in the Receiver, and did not froth at all.

We have a confirmation by this Experiment, That factitious-air is a greater enemy to Animals, than a *vacuum* is.

EXPERIMENT XIII.

June 22. 1678.

This-day in the morning I put green Pease into an empty Receiver, and towards evening the Mercury had almost attained to the height of 10 digits.

June 23.

The height of the Mercury was almost 30 digits.

June 24.

The Mercury did not as yet exceed 30 digits in height: The Cover did no longer stick to the Receiver, yet hitherto nothing had escaped out of it.

June

June 26.

I included the same Pease in the same empty Receiver.

June 29.

When I now found that the Receiver was filled with factitious air, I thrust a Snail into it, who put forth much spume or froth, and did very often expand and contract his horns; but after 6 minutes were elapsed, he lay still, as if he had been dead, for 2 or 3 minutes; then the Receiver being opened, and the Snail taken out, moved himself a little, if he were pricked; whence it seems to follow, that air produced from Pease is less prejudicial to Snails than air from Paste. See *Exper. XII, XI*. I blew new air into the Receiver, and a Snail then put into it did very well.

In this Experiment it seems observable, That Pease do quickly produce air *in vacuo*; but in the wonted compression of air they generate but little.

ARTICLE VI.

Animals in Vacuo.

EXPERIMENT I.

June 22. 1676.

I Put a Butterflie into an empty Receiver, and it was almost 3 hours before she was wholly deprived of her faculty of motion; at length, perceiving him to lie unmoved, I let in the air into the Receiver, and in a little time the Butterflie recovered his motion. Then I bound him by one of his horns with a thred, and so hanged him in the Receiver, and then he was carried very freely from one part of it unto the other, by clapping his wings; but after the air was extracted, the clapping
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of her Wings was in vain, for she could not move the thred in the least, from being perpendicular.

EXPERIMENT II.

July 12. 1676.

Yesterday I put 2 Flies into a Receiver, in which I left $\frac{1}{4}$ of air, (*i.e.*) as much as would sustain 10 digits of Mercury; The biggest of the Flies seemed to die presently, but the other, which was a small bodied one, lived almost 24 hours.

When both the Flies lay, as if they were dead, I suffered some air to enter in, till the Mercury was 15 digits high; and then the lesser Flie began to move her feet, but the other continued still without motion.

Hence it appears, That air highly rarefied may serve for Insects to breath in, and that it doth not kill them so soon as artificial air.

EXPERIMENT III.

May 1.

I put 2 Snails into an emptied Receiver, and for an whole hour they seemed to be well enough, and crept up to the top of the Receiver; but in 2 hours time, they fell down from thence, and lay without motion.

Six hours after they were first put in, I took them out *è vacuo*, and within half an hour they began to move a little. During the time they were included, they produced near as much air as sufficed to sustain the Mercury in the height of $\frac{1}{4}$ of a digit.

These Snails lived longer *in vacuo* than the others included in artificial air. *Artic. V. Exper. VI.*

E X P E R I M E N T I V.

August 12. 1676.

I put Fly-blowings, or the Eggs of Flies, into an empty Receiver, to trie, whether they would produce Worms there or no.

Aug. 14.

I saw the Worms were formed, but the air had crept into the Receiver, so that it could sustain 15 digits of Mercury.

Hence it appears, That Insects may be produced, and may live, if not *in vacuo*, yet at least in air very highly rarefied. See *Exper. VI, and VIII.*

E X P E R I M E N T V.

March 17. 1677.

I put 2 equal quantities of Frog-spawn into 2 Vessels of Glass, of equal bigness, I left the one included in an empty Receiver, exposed to the Sun; but the other, being in a Receiver full of common air, I fortified against the access of the external air. The Frog-spawn *in vacuo* did all swell into bubbles.

May 2.

No Frogs were produced in either Receiver, and that Seed or Spawn which was kept *in vacuo*, remained still full of bubbles; but about 3 days ago all the bubbles vanished, and the Spawn was changed into a certain green liquor.

July 2.

Our Receivers remained in a Window exposed to the Noon-day Sun; and so some Water that was mixed with the Frog-spawn, all *in vacuo*, and the very Spawn it self was elevated into vapours, and afterwards sticking to the sides of the Receiver, out of its own Vessel, was there condensed; but the Vessel kept in the common air, still contained all its Water, together with the Seed or Spawn.

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EXPERIMENT VI.

August 16. 1677.

I put Flies-Egs into an empty Receiver.

Aug. 29.

When no Worms were produced out of them, I gave admission to the Air to enter into the Receiver, and left all things in the same posture, to trie, whether the Eggs had lost their faculty of producing Worms.

Septemb. 9. The Eggs produced nothing.

This Experiment, if it be compared with *Exper. IV.* seems to shew, That Insects may be generated, and may live in air highly rarefied, but not at all *in vacuo*.

EXPERIMENT VII.

June 15.

I shut in a Frog in an emptied Receiver, at about 7 of the Clock in the evening, about 9 the Frog died.

June 16.

I repeated the same Experiment, and again perceived that the dead Frog in 2 hours space, had produced some air, rather than consumed it.

June 18.

The Frog, left hitherto *in vacuo*, was swollen very much; but the air now entering, made her far more flaccid and lank than she was wont to be.

We are instructed by this Experiment, That a Receiver void of artificial air, is less hurtful to the life of such kind of Animals. See *Exper. IV.* and *VII.* of *Artic. V.*

EXPERIMENT VIII.

August 3. 1678.

I put Flie-blowings sticking to Flesh, into an emptied Receiver.

Aug. 12.

No Worms were generated from them.

Aug. 15.

Perceiving no change in the Eggs, I opened the Receiver, to trie, whether they would yet be generated in the free air.

Sept. 15.

Nothing was produced from them.

We have a confirmation by this Experiment, That Animals, which may be generated and live in highly rarefied air, yet are killed *in vacuo*. See *Exper. IV*.

EXPERIMENT IX.

August 22. 1678.

I included Vinegar full of small Eels, or Vinegar-worms in an emptied Receiver.

Aug. 29.

The Worms were still moved, yet they were fewer than in the beginning.

September 6.

Yesterday some of those Worms did still move in our Vinegar, but this day I could not see one; whereupon taking a Microscope, I found them all dead; but in the Vinegar, which I had left in the open air, the Eels made as brisk motions as at the beginning.

Hence it appears, That those, even very diminutive Animals, are also affected with the presence and absence of the air.

ART I.

ARTICLE VII.

Fire in Compressed Air.

EXPERIMENT I.

May 14.

I Took a perfumed Cone, of that nature, that being once kindled in the Free air, 'tis wont by degrees wholly to be consumed; and put it into a Receiver firmly stopped with a Screw; and I intruded air into it, till the Mereury came to 120 digits above its wonted height, and then putting to my Burning glass, I kindled the Cone, which presently darkned all its Receiver with Smoke, and after sometime $\frac{7}{8}$ parts of 1 digit thereof in length were reduced to ashes; yet taking out the Cone, and blowing away the ashes, I found onely the superficies thereof consumed; but the inner parts were untouched.

I included another Cone of the same sort in a much greater Receiver, but I did not compress the air therein: The Cone, fired by the same Burning-glass, was not taken out, till all the Fumes were abated and fallen down; yet much less of this Cone was burnt than of the other.

EXPERIMENT II.

May 17.

I weighed a perfumed Cone exactly, and then firmly included it in a Receiver with common air, and I kindled it by the help of my Burning glass; when the Fumes were condensed, I took

took the Cone out of the Receiver, and weighed it again, the loss of its weight was almost one grain. Then I got me many pieces of Paper, each of them of the self-same weight, which I presume to call *Paper-grains*.

Afterwards the same Cone, observing the same circumstances, was again included and kindled, but first I had intruded air into its Receiver, as much as could sustain 90 digits of Mercury, and thus by means of a pair of Scales, I found the loss of weight this time was 4 times more than of the former, for the Cone was lighter by 4 Paper-grains.

From this Experiment it seems to follow, That the consumption of matter is so much the greater, by how much the greater quantity of air is contained in the Receiver.

EXPERIMENT III.

May 17. 1677.

I included a perfumed Cone in a Receiver firmly stopped by the help of a Screw; and, the air being compressed to sustain 60 digits of Mercury above its wonted pressure, I set fire to it with my Burning glass; the Cone being afterwards taken out, had lost 3 Paper-grains and an half in weight.

I repeated the same Experiment, but in air, so compressed, that the Mercury reached to 120 digits above the wonted pressure, then the Cone was $7\frac{1}{2}$ Paper-grains lighter; and so though the quantity of the air was not double, yet the consumption of the matter by the fire, was more than twice as much as that was in the former Experiment.

May 17.

I iterated the same Experiment in air, compressed to sustain 97 digits of Mercury, and then the loss of weight seemed to be 6 Paper-grains.

By all these Experiments we are taught, That the matter is so much the more consumed by the Fire, by how much the com-

compression of the air in the Receiver is the greater; yea, the consumption seems to have a greater proportion to the consumption, than the compression hath to the compression.

May 18. 1677.

I included a perfumed Cone as before, in a Receiver 7 times larger than that which I used in the former Experiments, and I immitted no air at all into it. The Cone kindled there, lost $3\frac{1}{4}$ Paper-grains of its weight, and no more; whereas in the same quantity of air, if it had been reduced to a 5 part of its space, the Cone would have lost 10 grains, *viz.* by observing the proportion of the consumption made before in air, sustaining Mercury to 120 digits above its accustomed height, (*i.e.*) air reduced to a 5 part of its space.

From this Experiment it seems to follow, That the same quantity of air, if it be reduced to less than its accustomed space, on that account alone causeth a greater consumption, than if it had remained in its wonted expansion.

EXPERIMENT IV.

May 19. 1677.

I repeated the Experiment last described, in the same Receiver, closely stopped with a Screw, that nothing might go out or in. The Cone lost 1 paper grain and a quarter onely of its weight, whence I suspect that it was not well kindled.

May 21.

I made the same Experiment, after the same manner. This day the Cone was lighter by 4 Paper-grains; whence I more certainly collected, That it was not well set on fire in the former Experiment.

May 23.

I repeated the same Experiment twice, but do suspect that
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the Cone was not well kindled, seeing at one time it lost only $\frac{1}{4}$, and at another time 1 Paper-grain of its weight.

May 24.

I tried the same Experiment again, and this day also the loss of weight was found only 1 Paper grain and a quarter. Then I opened my Receiver, and having wiped and cleansed away the Soot, I iterated the Experiment, and then the Cone took fire very well, for the loss of its weight amounted to 6 Paper-grains and an half.

I tried the same Experiment again in an uncleansed Receiver, and then the Cone lost only 3 Paper-grains in weight.

May 25.

I iterated the same Experiment in a Receiver well washed, and the Cone was lighter by 6 Paper-grains and an half.

I made the same Experiment in the like manner, and in a well cleansed Receiver, and the Cone lost 7 grains and an half of its weight.

I tried the same Experiment again, in an unwashed Receiver, and then I could not sufficiently kindle the Cone.

May 26.

I tried the same Experiment in an unwashed Receiver about the middle of the day, the Sun being clear, and clouded with no mists; and I removed not my Burning-glass from kindling the Cone a long time, so that it took fire very well, and became 8 Paper grains lighter.

By these Experiments it is manifest, That the quantity of a Cone to be consumed in the same quantity of air, is not fixed and certain, but sometimes greater, sometimes lesser, as the Cone shall be more or less kindled: Besides the imperfect mixture of the matter may cause some difference; yet it seems certain that fire is more easily kindled in compressed air, than in common; and the consumption will be the greater in a certain quantity of air, if that air be reduced into a narrower space, than if it enjoyed its wonted expansion.

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EXPERIMENT V.

May 22.

I put a perfumed Cone into a Receiver made for compressing the air; and intruding the air till the Mercury staid in 30 digits above its wonted pressure: I kindled the Cone, and found its weight to be abated $1\frac{1}{4}$ of a Paper-grain.

May 23.

I made the same Experiment again, after the same manner, and in effect with the same success.

I tried the same Experiment again, but the Cone took not fire well. Whence we have a confirmation, that Fire is more easily kindled in air much compressed, than in common air, or that which is but a little condensed.

I iterated the same Experiment, and after I had removed my burning-glass from kindling the Cone, whilst I was intent to see, whether the Cone would proceed to be consumed, the Receiver brake into 100 pieces, some of which struck my head and wounded it: which passage I mention, that so no man may be confident his Glass will not break, whilst he is about these Experiments, because he hath found that at other times it hath resisted a greater pressure. For this very Glass of mine, had contained air 4 times more compressed, very well. See *Exper.III.* Yea in *Exper.VI.* of *Artic.II.* it had resisted Air, sustaining 198 digits of Mercury above its wonted height; yet now it was broken by a pressure more than 6 times less: and therefore whilst a man looks into such Receivers, his head had need be fortified with some perforated or pellucid muniment and defence to preserve it from a blow.

ARTICLE VIII.

Fire used to produce Air.

EXPERIMENT I.

June 4. 1676.

I Burnt Paper, besmeared with Sulphur *in vacuo*, and found that it produced some Air, which Air was not at all diminished for 2 whole days.

That Air is to be ascribed to the Paper, for no Air is produced out of Sulphur alone.

EXPERIMENT II.

June 15.

I burnt Harts-horn *in vacuo*, and found that the Fumes issuing therefrom, did contain some Air in them.

June 17.

These 2 last days, I iterated the same Experiment, and always observed, That, Air produced from Harts-horn, was in a short time in part destroyed; but that, which preserved the elastick nature of Air for a full hour after the Burning-glass was removed, seemed afterwards not to lose it at all.

June 19.

I took the Harts horn out of the Receiver, and found no volatile Salt, but onely a fetid Oil to be produced therefrom.

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EXPERIMENT III.

June 21.

I burnt Amber *in vacuo*, and at first I could not find that the Fumes did ascend above the height of one digit; and yet in a Receiver full of Air, they would be carried up to the top of the Receiver, and from thence be reflected downwards; yet afterwards, even in the *vacuum* it self the Fumes reached almost to the top of the Receiver, but the Mercury was not at all changed in its Gage.

June 22.

This night, a great deal of that Water, in which I had immersed the Receiver, found a passage into it, though the Cover was so well fitted to the aperture, that I never perceived any water to get in betwixt them before. Hence a suspicion arose in me, that some volatile Salt had probably attracted (if I may so speak) the aqueous parts, by reason of the congruity betwixt them.

July 8.

I still kept the Receiver immersed in Water, but no more Water entered in, as if, the Salts being washed away, the external Water, being destitute of assistance, could no longer creep in: But that agreement between the Fumes of the Amber, and the parts of the Water had need of a confirmation by a great many more Experiments.

Hence it appears, That Amber produceth no Air, no not though it be burnt.

EXPERIMENT IV.

Jan. 18. 1677.

I put 2 drachms of Camphire into an empty Receiver, and the commissure of the Cover with the Receiver, being fortified

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against external Air. I put the Camphire on a digesting Furnace.

Jan. 19.

The Camphire was sublimated into Flowers, but no Air was produced.

EXPERIMENT V.

May 24. 1676.

I included *Sulphur vivum* in an exhausted Receiver, and melted it by the help of my burning-glass, but found that the Fumes produced therefrom, did contain no Air in them, because the Mercury did ascend to the aperture of its Gage, as it useth to do while the Receiver is evacuating: yet when the Receiver was cooled, the Mercury returned to its former height; and therefore I think that change proceeded onely herefrom, because the Air included in the sealed leg of the Gage, was rarefied, and drove the Mercury into the other part.

EXPERIMENT VI.

July 19.

Having included Paste 9 days agoe *in vacuo*, and perceiving that it now contained no more air; I endeavoured to fire it with my burning glass. The subsiding Fumes had tinged the superficies of the Paste, with a curious yellow colour; and besides I conjectured, That some Air was produced, because the Receiver, which before was straitly joyned to its cover, was now with ease plucked therefrom.

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ARTICLE IX.

Concerning the Production of Air in Vacuo.

EXPERIMENT I.

September 9. 1676.

I Exhausted the Air out of a Receiver half full of dried Grapes, and fortified it against the external Air.

Sept. 10.

In 24 hours time the height of the Mercury was $\frac{1}{2}$.

Sept. 12. In two days time, the ascension of it was $\frac{1}{2}$.

14. The ascension of the Mercury was $\frac{1}{2}$.

17. The ascension of it was $\frac{1}{2}$.

22. The ascension of it was $\frac{1}{2}$.

27. The ascension was $\frac{1}{2}$. The height 3 digits.

October 11.

The height of the Mercury was now about 6 digits.

September. 9. 1676.

I put dried Figs into a Receiver, and filled about half of it with them, and then I extracted the Air, till the Mercury staid in the height of 3 digits.

Sept. 10. No Air was produced.

Sept. 17.

Perceiving no Air to issue out of the Figs, I opened the Receiver.

By this Experiment we learn, That dried Fruits, put into an exhausted Receiver, do produce very little Air with any regularity.

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EXPERIMENT II.

August 5. 1676.

I included Pears and Apricocks *in vacuo*.

August 6.

In 18 hours time the Mercury reached 2 digits; in 10 hours more it reached the third digit. Its height was 3 digits.

Aug. 7. The height of it was 5 digits.

8. The height of it was $6\frac{1}{2}$.

9. In 14 hours space, the Mercury mounted $\frac{1}{4}$. Its height was $7\frac{1}{4}$.

Aug. 10	$\left. \begin{array}{l} 11 \\ 12 \\ 13 \\ 14 \\ 15 \\ 16 \end{array} \right\} \begin{array}{l} \text{The height} \\ \text{of it was} \end{array}$	$\left. \begin{array}{l} 8\frac{3}{4} \\ 10\frac{1}{4} \\ 12\frac{1}{4} \\ 14\frac{1}{4} \\ 16 \\ 18 \\ 20 \end{array} \right\}$	Aug. 18	$\left. \begin{array}{l} 19 \\ 20 \\ 21 \\ 22 \\ 23 \\ 26 \end{array} \right\} \begin{array}{l} \text{The height} \\ \text{of it was} \end{array}$	$\left. \begin{array}{l} 25 \\ 29 \\ 31\frac{1}{2} \\ 32\frac{1}{2} \\ 34 \\ 35 \\ 38\frac{1}{2} \end{array} \right\}$

Aug. 29. The height of the Mercury was 41.

Sept. 1. The height of the Mercury was $42\frac{1}{2}$.

4. The height of it was 44.

7. The three days last past, being hotter than the foregoing, the ascension of the Mercury was $2\frac{1}{4}$. Its height was $46\frac{1}{4}$.

Sept. 10. The height of the Mercury was $47\frac{1}{2}$.

13. The Mercury was depressed, its height was only 44 digits.

23. The Mercury was by degrees again mounted to the 48 digit.

27. The height of the Mercury was $50\frac{1}{2}$.

Nov. 5. The Mercury ascended by degrees to the height of $52\frac{1}{2}$.

Nov.

Nov. 28.

The Apricocks were reduced to Water; the skin was severed from the Pulp, yet no more Air was produced.

Jan. 10. 1677.

Whilest it was a very hard Frost, the Mercury came to the height of 57 digits: but when the Thaw came, it was depressed to 23. Whether the strength of the Frost opened some way for the Air to get out, I know not.

March 3.

The Mercury could ascend no higher, because the Air was got out. This day I found the Receiver tumbled on the ground, and the Apricocks, when the Frost was broke, were putrified, and had lost their colour.

From this Experiment it seems to follow, That Apricocks do produce Air almost as easily in their wonted pressure, as *in vacuo*.

EXPERIMENT III.

June 20. 1676.

I put sower Cherries into 2 empty Receivers, and observed altogether the same circumstances in them both; save that in the one, the Cherries were *whole*, in the other, *cut* asunder. In 2 hours space the *whole* Cherries had impelled the Mercury into the Gage to the height of 10 lines; and the dissected Cherries, to about 20.

June 21.

In 24 hours space, the Mercury, which was in the Receiver, containing the *whole* Cherries, came to the height of 3 digits; but in the other Receiver the Mercurial Gage was spoiled.

June 26.

The *whole* Cherries had not yet produced so much Air that could sustain 15 digits of Mercury; but the dissected Cherries had wholly filled their Receiver with Air.

July

July 9.

This day the Receiver of the whole Cherries was removed from his Cover: I did eat one of the Cherries, and its taste seemed pleasant enough. I included the rest again *in vacuo*, many of them were broke, and in one hours space they impelled the Mercury to ascend to the height of about 2 digits.

July 10.

These last 24 hours the Mercury ascended not; whether the Gage was prejudiced, I am not certain.

July 15.

This day I found the Cover severed from his Receiver, and so it was clear, that the Gage was spoiled or hurt.

This Experiment gives us a probable consequent, That some *dissected* Fruits do sooner produce their Air, than *whole* and undivided ones.

EXPERIMENT IV.

June 9. 1676.

I put Cherries (not acid ones) into an empty Receiver, and within one hour I found as much Air produced from them, as sufficed to sustain $\frac{1}{4}$ of a digit of Mercury.

June 10.

In 18 hours the Mercury seemed to have come to the height of 11 digits.

June 11.

Our Fruits produced Air, less, and less copiously; so that this day, towards the evening, they came not up to the height of 15 digits.

June 12. Now the Mercury was a little higher than 15 digits.

13. The height of the Mercury was 22 digits.

16. The Mercury yet came not up to 30 digits.

18. Perceiving no more Air to be produced from my Fruits, I opened the Receiver.

Such

Such a small production of Air seemed very observable to me, because I had found by experience, that Fruits of the same kind in *France*, had filled their Receiver in 2 days time; it may probably come to pass, that Fruits of the same kind, in several Countries, may differ much amongst themselves.

EXPERIMENT V.

June 12. 1676.

I put Cabbages cut in pieces into an empty Recipient, with a Mercurial Gage, and in one hours space the Mercury had made one line.

June 13. The Mercury was now come almost to the height of 10 digits.

17. The Mercury was come almost to the top of its Gage, and the Receiver being opened, I found the Cabbages little altered.

19. The Cabbages being left 2 days in the open Air, were wholly corrupted and blackish. I put them again *in vacuo*, to trie, whether the putrefaction begun, would promote, or else retard the production of Air.

June 19. The Mercury in half an hour ran up $\frac{1}{2}$ of a digit.

22. For three whole days the Mercury got higher onely 10 lines. Its height was 1 and $\frac{1}{4}$ of a digit.

23. Finding that the Cabbages produced no more Air, I took them out of the Receiver, their Smell was very bad.

Hence a suspicion arose within me, That Bodies, when they putrefie, have already produced almost all their Air.

EXPERIMENT VI.

May 29. 1676.

I took pieces of Orange weighing 4 ounces, and put them into a Receiver capable of holding 10 ounces of Water, and I exhausted the Air.

Q

June

*The Second Continuation of**June 10.*

This day the Receiver was removed from his Cover, by the force of the produced Air; so that I took out the Oranges, and presently put them into another empty Receiver capable of containing 8 ounces of Water, and the Mercury within half an hour, was elevated to the height of one half digit.

June 13.

That sudden ascension of the Mercury was not durable, for it yet came not to the height of 2 digits.

June 16.

The Mercury, the last 24 hours ascended about 3 lines.

June 21.

The Mercury, these last 24 hours, did not ascend the space of one line.

July 18.

I perceived no more alteration was made in the height of the Mercury; but some mouldiness appeared, though I am certain that no Air from without, had found any ingress into the Receiver.

E X P E R I M E N T VII.

April 27. 1676.

I put a Tulip into an empty Receiver, with a Mercurial Gage, but before it was fortified against the external Air, some Air had got in, enough to sustain 2 digits of Mercury.

May 2.

The Tulip, which first seemed striped with sundry colours, was now wholly changed into a dark red, and was moist, It produced very little Air.

E X P E R I M E N T VIII.

April 22. 1676.

I put half of a Limon into an empty Receiver, with a Mercurial

curial Gage, so short, that the Mercury could not run up the space of 3 digits.

April 24. In 2 days space the Mercury came to the height of one digit and an half.

25. The Mercury was now 2 digits high.

27. Yesterday the Mercury made 4 lines, but this day only one.

29. The 2 last days, the Mercury mounted higher by one line.

May 3.

In 4 days space the Mercury ascended one line and a little more.

May 3. 1677.

The Mercury came to the top of its Gage, yet no Air got out; but the Limon was little altered.

Jan. 1. 1678.

As yet no Air escaped out of the Receiver; but the Limon had contracted a yellow colour, and moisture therewith.

EXPERIMENT. IX.

March 16. 1677.

I put 2 Apples, of the same sort, in 2 empty Receivers, one of the Apples began to putrifie before, the other was only bruised with a few blows.

May 15. 1677.

As yet the Fruits were in very good case; but this day that Apple which was bruised, appeared wholly rotten, and the Receiver was forced from his Cover; the other Apple remained without any change.

August 20. 1677.

That Apple which before began to be rotten, suffered no farther alteration; but this day finding that the Receiver was pulled from his Cover, and fearing lest the Apple would be speedily putrified, I took it out; its taste was grateful, but sub-acid, as if it had been fermented; but the pulp inclined to the consistence of meal.

Q₂

From

From this Experiment it seems to be confirmed, That Fruits have produced the greatest part of their Air, when putrefaction begins to alter them; seeing the putrid Apple did not fill its Receiver but in a much longer time than the other Apple. See *Exper. V.* of this Article.

E X P E R I M E N T X.

May 17. 1676.

I poured 2 equal quantities of Milk into 2 Glass Receivers, of equal bigness; the one I left in the Free Air, the other I included to be kept in an emptied Vessel, with a Mercurial Gage.

May 18.

The Cream did swim on the top of that Milk, which was left in the Free Air; but that which was *in vacuo*, was onely covered with Bubbles; and the Gage was not changed at all.

May 19.

The Bubbles swelled more and more, and the Mercury in the Gage was a little higher.

May 20.

The Bubbles *in vacuo* swelled yet more, and that Milk seemed curdled; but the other in the Free Air was manifestly curdled. The Mercury *in vacuo* came almost to the top of its Gage.

May 22.

The Milk *in vacuo* proceeded to generate Air more and more, and now it evidently appeared to be curdled; whence it is manifest, that the coagulation of Milk, when the Air is taken away, is retarded. Now almost all the Bubbles were broke.

June 20.

The Milk *in vacuo* was no longer covered with Bubbles, and remained still coagulated in the same state. But the Milk in the Free Air, stank filthily, and was full of Worms: when it was put on the Engine, and the Air extracted, it did emit many

ny very great bubbles for a long time; and the Worms did move themselves very vehemently, but not one of them died in 4 hours space.

May 19. 1677.

Three or four Moneths ago, some Whey *in vacuo* was poured out of a Vessel into a Receiver, and it seemed clear and limpid, like Water; yet there was Whey enough left in the Vessel, to separate the Butyrous from the Caseous part, at a sufficient distance.

This day the Milk stagnant in the Receiver, seemed to have got out of it; so that it is clear, that the Air in the Receiver, was of greater force than the external Air, for the Cover also was forced from the Receiver. Towards night, I took that Milk out of the Receiver, and found it to be acid, both in smell and taste, yet it was not unacceptable to the palate; but after a short time, the Whey, which hitherto had remained limpid between the Caseous and Butyrous part, began to disappear, and to be blended with the rest.

May 24.

This day the Butyrous part was wholly vanished though as yet it had suffered no sensible mutation; but the Milk began to smell amiss.

June 1.

Our Milk had not yet contracted the worst of smell, neither had it produced any Worms, but it grew dry by degrees; and this night the Mice eat it up, as perhaps they had done the Butyrous part before.

This is the Story of my Preserved Milk, in which these 4 things seem most observable. First, That the Coagulation of Milk, when Air is extracted therefrom, is somewhat retarded. Secondly, The weight of Butter, or of Whey, or Cheese, is not the same in the Air, as it is *in vacuo*; for in the Air they are mixed one with another confusedly: but *in vacuo* one swims
on

on the top of the other. Thirdly, The putrefaction of Milk, when Air is extracted, is hindered, or very much retarded. Fourthly and lastly, Milk by long continuance *in vacuo*, is made unfit to generate Worms, even in Common Air.

EXPERIMENT XI.

September 5. 1677.

I took the same Receiver, and the same Vessel, which I used before to preserve Milk *in vacuo*, and I included Urine therein, after the same manner, as I had done Milk before. The quantity of Urine was 3 ounces and 3 drachms, or thereabouts; and the Receiver was onely capable of holding 10 ounces of Water.

Sept. 7.

The Mercury reached to the height of almost 2 digits.

Sept. 8.

The Mercury was this day somewhat higher than yesterday.

December 5.

The Mercury ascended not above 3 digits in height, and for the whole moneth past was not changed at all. The Urine seemed not at all to be altered.

Decemb. 6.

I set other Urine under a Receiver, not fortified against the external Air.

Decemb. 16.

The Urine *in vacuo* still kept unaltered, but the other, in 10 days time seemed turbid, and to have contracted some mouldiness in its superficies.

This Experiment, compared with the former, gives us a probable inference, That Urine, which is an excrementitious humour, contains less Air in it, than Milk which is alimental.

Moreover, The efficacy of the Air to corrupt Urine, seems very observable.

EX-

EXPERIMENT XII.

May 19.

I took Paste very much diluted, and without Leaven, and put it in a Glafs Vessel into an empty Receiver; and though the Vessel, which contained it, were not half full, before all the Air was exhausted, yet the Paste had swollen above the brims of the Vessel.

May 20. The Paste continued to swell more and more, and was interspersed with many cavities.

May 22.

This day the Paste was much more tumid than before, and much Air was generated therefrom.

May 23.

This day in the morning I found the Cover severed from his Receiver, by the force of the produced Air, and some of the Paste was spread above the edges of the Receiver, yet its swelling was somewhat abated. In the afternoon, its tumidness was much more abated, yet it took up twice more room than it did before it was put into the Receiver. The taste of it was not acid, and therefore I think that Bread, thus made, is very light.

EXPERIMENT XIII.

July 20. 1676.

I took a quantity of Beef, and put it into an exhausted Receiver, fortified against the external Air; and likewise I put another equal quantity of Beef into a Receiver, neither exhausted, nor closely stopped.

July 21.

In 30 hours space, the exhausted Receiver was all filled with Air, so that I suspected some Air had got in; and therefore I in-

included the same Beef again, and so closed it, that there was no fear of the ingress of any external Air.

July 22.

In 14 hours space the Mercury came to the height of 15 dig.

July 25.

For 3 whole days and more, the Beef did not produce so much Air, as would fill one half of the Receiver.

July 26.

This day the Receiver was severed from his Cover; and in one hours space, I perceived that the Beef, being again included *in vacuo*, had produced Air, which sufficed to sustain 10 digits of Mercury.

July 28.

I found the Receiver again filled with Air, and re-exhausting it, much Air was in a short time again produced from the Beef.

July 30.

The Receiver being again filled, I included the Beef again *in vacuo*, and found, that the Air produced from it in one hours space, was able to sustain 10 digits of Mercury.

August 1.

The Receiver being this day filled again, the Beef stank so filthily, that we threw it out of doors.

Hence it appears, That Flesh, whilst it putrifies, doth produce much more Air, than before it putrifies; but 'tis otherwise with Fruits. See *Exper. IX.* of this *Artic.*

EXPERIMENT XIV.

July 18. 1676.

I put some Goosberries, which I had kept long in Receivers to produce Air, into a vacuous Receiver.

Within half an hour the Mercury ascended to the height of one digit.

In an hour and halfs time, the Mercury mounted another digit.

July

July 19.

In 24 hours time, the Receiver was almost all filled with Air.

July 20.

The Cover was forced from his Receiver, and much juice had run out of the Receiver.

July 29.

I left the same Goosberries in a Receiver, not hitherto fortified against the external Air; but this day I included them again *in vacuo*, to trie, whether they could produce any more Air.

July 30.

In 16 hours time, the Goosberries drave up the Mercury a digit and $\frac{1}{2}$ into the Gage.

July 30. 1677.

The Goosberries could not wholly fill their Receiver; and they always remained in the same state, but a while since they had almost lost their red colour, and inclined to white.

From this Experiment it seems to follow, That these Fruits, after they have produced all their Air, admit very little alteration; as if that Air it self were the cause of corruption.

EXPERIMENT XV.

August 23.

I put Pears into a *vacuous* Receiver with a Mercurial Gage; and before the Receiver could be well fortified against the ingress of the Air, the Mercury was come to the height of one digit and an half.

In 2 hours space the Mercury ascended 4 digits; its height was almost 6.

August 24. The height of the Mercury was 12 digits.

25. The height thereof was 16.

R

Aug.

The Second Continuation of

Aug. 26 } The height { 18 | *Aug.* 28 } The height { 23
 27 } of it was { 21 | 31 } of it was { 30

Sept. 1 } The height { 32 | *Sept.* 4 } The height { 44 $\frac{1}{2}$
 2 } of it was { 35 | 5 } of it was { 45 $\frac{1}{2}$
 3 } of it was { 38 $\frac{1}{2}$ | 6 } of it was { 50

Sept. 7. The height of it was the same, because some Air had escaped, but I prevented that for the future.

8. The height of the Mercury was. 53 $\frac{1}{2}$.

9. The height of it was 54 $\frac{1}{2}$.

10. The height of it was 58.

Septemb. 12.

Yesterday the Mercury persisted in the same height; but this day it seemed to be depressed: whence I conjecture, that some Air had got out. The height of it was 53 $\frac{1}{2}$.

Sept. 13.

I transmitted the Air into another Receiver: the height of it was 32 $\frac{1}{2}$.

Sept. 16.

I perceived that the Air had got out; and opening the Receiver, I found the Pears very rotten.

These Pears produced their Air irregularly enough, sometimes quicker, sometimes more slowly.

E X P E R I M E N T XVI.

September 17.

I put dried Plums into an evacuated Receiver.

Sept. 19. The Mercury seemed to have ascended a little.

22. I perceived not that the height of the Mercury was any more altered.

Novemb. 8.

When I saw that the Plums produced no more Air, I opened the Receiver.

By

By this Experiment, we have a confirmation, That dri'd Fruits are very unfit to produce Air.

EXPERIMENT XVII.

Septemb. 28.

I put fresh Nut-kernels, cut into pieces, having thrown away their shells, into an evacuated Receiver with a Mercurial Gage.

29. The Mercury ascended a little.

30. The height of it was 2 digits.

Octob. 5.

The Mercury proceeded to ascend by degrees : the height of it exceeded 6 digits.

Oct. 15. The height thereof was 10 digits.

22. The height of it was 15.

Nov. 28.

The Mercury was come to the height of 20 digits, or a little more ; but this day the Receiver was cast down and broken, and the Nut-kernels thrown about ; they were kept very well, both as to colour and taste.

Hence we may conjecture, That Air without sensible putrefaction may be produced from Fruits, even of an hard consistence.

ARTICLE X.

Concerning the Production of Air above its wonted Pressure.

EXPERIMENT. I.

June 22.

Included new Pease in a Receiver with a Glas full of Raisins of the Sun bruised, and mixed with Water, I did not exhaust the Air.

Towards Evening the Mercury had mounted to 12 digits, but a great part of that Air was produced from the Raisins, not from the Pease.

June 23. The height of the Mercury was 49.

June 24 } The height { 75 | *June 26* } The height { 90
 25 } of it was { 90 | 28 } of it was { 100

The Pease did as it were sweat, and grow yellow.

30. The height of the Mercury was 110.

July 1. The Mercury ascended not, yet no Air escaped out.

4. The height of the Mercury was 124.

7. The height of it was 140.

July 10.

The height remained the same, but the liquor which distilled, or sweat out from the Pease, got out.

July 12.

New liquor was produced from the Pease, but the Mercury continued in the same height.

July 13.

The liquor got out of the Receiver, and some Air besides; where

whereupon I *set* the Screw, and new liquor being in a short time collected, did fortifie the Cover within.

July 15.

This day the Receiver was broken in pieces ; but the Pease being softer than ordinary, were easily stript of their husks, as if they had begun to be boiled : they kept their ordinary taste.

EXPERIMENT II.

Sept. 15. 1676.

I put unripe Plums into a vacuated Receiver ; but before the Receiver could be guarded against the external Air, the Mercury had already ascended to the height of one digit.

Sept. 16.

In 24 hours time the Mercury ran up 5 digits, its height was 6 digits.

Sept. 17. The height of the Mercury was 8.

<i>Sept. 18</i>	} The height of it was	$\left\{ \begin{array}{l} 10 \\ 12 \\ 14 \\ 18 \end{array} \right\}$	<i>Sept. 23</i>	} The height of it was	$\left\{ \begin{array}{l} 18 \\ 19 \\ 23 \\ 26 \end{array} \right\}$
19			24		
20			26		
22			28		

Octob. 1. The height of the Mercury was 30.

4. The height of it was 31. 'twas somewhat cold.

<i>Octob. 5</i>	} The height of it was	$\left\{ \begin{array}{l} 32 \\ 33 \end{array} \right\}$	<i>Octob. 9</i>	} The height was 33 $\frac{1}{2}$.
7			11	

Octob. 15. These 2 last days, the Cold being abated, the Mercury ascended more speedily ; its height was 37.

<i>Octob. 17</i>	} The height of it was	$\left\{ \begin{array}{l} 38 \\ 39 \frac{1}{2} \\ 41 \\ 43 \end{array} \right\}$	<i>Octob. 29</i>	} The height of it was	$\left\{ \begin{array}{l} 45 \\ 46 \\ 47 \\ 53 \end{array} \right\}$
19			<i>Nov. 2</i>		
22			5		
26			20		

In this Experiment, the Air seems to be produced sometimes regularly enough, and at other times Anomalously.

EX-

EXPERIMENT III.

July 6. 1676.

I put Goosberries into an emptied Receiver, but before it could be guarded against the external Air, it had entered in, and impelled up the Mercury to the height of half a digit; and afterwards in half an hour, the Air produced from the Goosberries, had impelled it up to another semi digit.

In 7 hours time the Mercury ascended 4 digits higher: it staid in 5.

July 7. In 14 hours space the ascension of the Mercury was 2 digits and $\frac{1}{2}$.

In 10 hours space, the ascension of it was 2 $\frac{1}{2}$.

July 8. In 14 hours the ascension of the Mercury was 1 $\frac{1}{2}$.

In 10 hours the ascension of it was 2 digits.

July 9. In 14 hours the ascension of the Mercury was 2 $\frac{1}{2}$.

In 10 hours its ascension was 1 $\frac{1}{2}$.

July 10. In 14 hours the ascension of it was 1 $\frac{3}{4}$.

In 10 hours the ascension of it was 3.

July 11. In 24 hours the ascension of the Mercury was 4.

July 12. In 24 hours the ascension of the Mercury was 4.

Now the Mercury was brought to its wonted pressure.

July 13.

This day in the morning, I found the Cover to be broken, and because it was fastned by a Screw, that it might not be severed from the Receiver, I suspected that it was broken by the force of the internal Air; I substituted another Cover in its place.

July 14, 15, 16, 17, 18.

I perceived no change in the height of the Mercury, because the Cover was not exactly shut; and therefore I took out the Fruits, and put some part of them into another evacuated Receiver, and the rest I stopped up closely with common Air, that nothing might get out.

In

In 4 hours the ascension of the Mercury was 4 digits.

July 19. In 14 hours the ascension of the Mercury was $1\frac{1}{2}$. but, suspecting the Air to have escaped, I *set* the Screw.

In 9 hours the ascension of the Mercury 11 digits.

The Cover was broke, and the Air made an escape.

This Experiment seems to prove, That Goosberries contain much Air in them, which, as soon as it is freed from the wonted presson of the Air, doth more readily break forth, than when it is restrained by some ambient Air, until the Goosberries begin to be fermented, for then Air is produced in a far larger quantity, even in a great compression.

EXPERIMENT IV.

July 8. 1676.

I included Paste in an exhausted Receiver, and, before it was guarded against the external Air, the Mercury was come to the height of 3 digits, by reason of the Air making an irruption from without; whence it came to pass, that the Paste, which was much swollen, lost about the third part of its tumidity.

A little while after it swelled again, and within half an hour the Mercury mounted higher by 2 digits.

In one hours time the ascension of the Mercury was $2\frac{1}{2}$. and the Paste continued to swell or rise more and more.

In another hours space the ascension of the Mercury was 3 digits and $\frac{1}{2}$.

In 1 hours time the ascension of it was $4\frac{1}{2}$ digits: it staid in 16.

July 9.

In 14 hours space, the ascension of it was 21 digits. The height of the Mercury was 37. Moreover I suspected that some Air had got out; when I *set* the screw, the Cover brake, and upon the ingress of the external Air, the Paste, which always did *rise*, now did abate about 2 digits of its tumidity, though it was now found in a less compression than before.

In

In 5 hours space the ascension of the Mercury was 15 digits. But when I again endeavoured to *set* the Screw, the Cover brake, so that the Air escaped; the Paste did presently somewhat pitch, and was depressed.

In 4 hours space the ascension of the Mercury was 10 digits, the Paste did again swell or rise, as before; but being willing to substitute a better Screw in the place of the other, I permitted an egress to the Air, yet this time the Paste did not pitch or subside, as before it had done.

July 10.

This night the Paste *rose* again, yet it seemed to have produced no Air.

In 4 hours space there was no ascension of the Mercury.

In 7 hours space the ascension of it was 4 digits.

July 12 I perceived no ascent of the Mercury.

13. It seemed to have ascended a little.

17. Seeing no more Air was produced, I took out the Paste and found it to be of a subacid smell,

This Experiment seems to prove, That Air may be produced out of Paste, in *compressed* Air, as well as in *vacuo*.

But the Paste was twice depressed, because the compressed Air suddenly finding out a way of eruption, was so much dilated, as it is wont to happen in all Springs, when they are carried beyond their point of rest; but, when that Air was immediately repelled by the external Air, the Paste did pitch and was depressed.

EXPERIMENT V.

July 13. 1676.

I included some Beans, of that sort which are given to Horses for Provender, in *vacuo*, with some Water; some of them which were *bruised*, seemed to swell much; but those which were left *whole*, suffered no sensible alteration.

In

In 2 hours space I saw no Air produced, though the Beans continued to swell.

July 14. In 24 hours the ascension of the Mercury was 7 digits.

July 15. In 16 hours the ascension of the Mercury was 3 digits and $\frac{1}{2}$.

In 8 hours the ascension of it was $1\frac{1}{2}$. the height of it was 12.

July 16. In 14 hours the ascension of it was 3.

17. In 26 hours the ascension of it was 6.

18. In 24 hours the ascension of the Mercury was almost 9.

19. I stopped the Receiver firmly with a Screw, because the Air had got out. In 9 hours space the ascension was 1 digit.

20. In 24 hours space, the ascension was $3\frac{1}{2}$.

21. In 24 hours space the ascension was $5\frac{1}{2}$.

22. In 14 hours the ascension of the Mercury was 2 digits.

23. In 24 hours the ascension of the Mercury was 18 digits.

24. In 14 hours the ascension of the Mercury was almost 5. The height of it was 35 above the wonted pressure.

25. The Receiver could not sustain a greater pressure. I found the Beans of a foetid smell, not much unlike the smell of putrified Flesh.

From this Experiment it seems to follow, That Beans contain much Air in them, and that, that Air is produc'd in a moderate pressure, as well as *in vacuo*, sometimes more speedily, sometimes more slowly.

Especially, that great inequality, which happened July 23. is to be taken notice of.

EXPERIMENT VI.

July 23.

I included Goosberries *in vacuo*, and fortified them very well against the external Air.

In 2 hours space the Mercury ascended 1 digit.

July 24. The height of the Mercury was 7 digits $\frac{1}{2}$.

July 25	} The height 26	} of it was	{ 12 17		July 27	} The height 28	} of it was	{ 20 24 $\frac{1}{2}$

July 29. The height of it was almost 30.

30. The height of it was almost 31. I transmitted some Air out of this Receiver into another evacuated Receiver, and so the height of the Mercury was 26.

31. The height of the Mercury was 35.

August 1.

The height of the Mercury was 39. But some Air had escaped out; and going about to stop the Receiver close, I suffered some more Air to get out.

The height of the Mercury was 30.

Aug. 2. The height of the Mercury was 39. I transmitted some Air into another Receiver.

The height of the Mercury was 31.

Aug. 3. The height of the Mercury was 39.

4. The height of the Mercury was 41.

5. The height of the Mercury was 43. I transmitted the Air into another Receiver.

The height of the Mercury was 30 digits.

6. The height of the Mercury was 43.

7. The height thereof was 47.

8. The height thereof was 48. But the Air being transmitted into another Receiver, the height of it was 36.

9. The height of the Mercury was 41. Fourteen hours were past.

Aug.

Aug. 10. The height of the Mercury was 47. the Air being transmitted into another Receiver, the height of it was 35. 24 hours were elapsed.

11. The height of the Mercury was 38½. Fourteen hours were elapsed.

12. The height of the Mercury was 42. twenty four hours were passed. I extracted the Air, and the height of the Mercury was 26.

13. The height of the Mercury was 33. twenty four hours were elapsed.

14	}	The height	{	36	}	hours		17	}	The height	{	44	}	hours
15				39				18				47		
16				41½				19				50		

of it was 24. of it was 24.

I transmitted the Air into another Receiver, and the Mercurial Gage was spoiled. I took out the Goosberries, and found that they had lost their colour, and also almost all their acidity.

From this Experiment we may infer, That Goosberries do produce their Air regularly enough, unless something be extracted out of the Receiver, for then they acquire strength to produce new Air more speedily.

EXPERIMENT VII.

September 12.

I put crude Grapes into an emptied Receiver, but before they could be fortified against the external Air, some thereof had got in, as much as could sustain 3 digits of Mercury.

<i>Sept.</i> 13	}	The height	{	5	}	<i>Sept.</i> 17	}	The height	{	19	}
14				10						19	
16				17						20	

of it was of it was

Sept. 22. The height of the Mercury was 30. I stopped the Receiver with a Screw.

23 The height of the Mercury was about 30½.

24 The height thereof was 32.

S 2

Sept.

Sept. 26	} The height of it was	34 $\frac{1}{2}$	Octob. 2	} The height of it was	39 $\frac{1}{2}$
27		36 $\frac{1}{4}$	4		39 $\frac{1}{2}$
28		36 $\frac{1}{4}$	5		40 $\frac{1}{2}$
29		37 $\frac{1}{4}$	7		41 $\frac{1}{2}$
30		37 $\frac{3}{4}$	9		42 $\frac{1}{2}$

Octob. 15. The height of the Mercury was 46. It ascended chiefly these 2 last days, when the Frost was dissolved.

Nov. 2. The height of the Mercury was 54 digits.

5. The height was 58.

Jan. 10. 1677.

Now the Mercury was come to the height of 70 digits; and yet I perceived no sensible mutation in the Mercurial Gage, even when the Cold was most fierce, though the Grapes and their Juice were concentered into Ice.

September 21.

Hitherto the Grapes seemed not altered: but the Mercury had ascended a little, because the Air had found a passage out. This day I opened the Receiver, and when the Air brake forth, many of the Grains seemed to be contracted into wrinkles. The Grapes had kept their taste but much more pungent; but their Juice continued to be tinged with a curious red colour.

This Experiment seems to inform us, that Grapes produce not all their Air, but in a long tract of time.

EXPERIMENT VIII.

August 10. 1677.

I put Pears cut in two, into a vacuous Receiver. Towards Evening the Mercury was come up to the height of 10 digits.

Aug. 11	} The height of it was	20	Aug. 15	} The height of it was	55
13		38	16		60
14		48	17		68

The

The Air being transmitted into another Receiver, the height of the Mercury remained at 53 $\frac{1}{2}$.

Aug. 18	} the height { 61		Aug. 20	} the height { 70
19			21	

The Air being transmitted into another Receiver, the Mercury remained in the height of 61.

Aug. 22	} the height { 68		Aug. 24	} the height { 79
23			25	

The Air being transmitted into another Receiver, the height of the Mercury was 61.

Aug. 26. The height of the Mercury was 56. because some Air had got out, yet I transmitted the Air into another Receiver, and the Mercury remained in the height 52.

Aug. 27	} the height { 60		Aug. 30	} the height { 83	
28			31		} of it was { 88
29			Sept. 1		

Septemb. 2. The height of it was 100.

Sept. 3. The height of it was 89. because some Air had escaped out, which made me cautious to prevent the like for the future.

Sept. 4. The height of the Mercury was 100.

5. The same height continued.

7. The same height still continued, though no Air at all had any egress.

9. The height of the Mercury was 107.

10. The height of the Mercury was the same.

The Air being transmitted into another Receiver, the Mercury staid in the height 99.

Sept. 11. The Mercury moved not.

13. The height of the Mercury was 105.

October 8. I this day found that the Air had got out.

This Experiment seems to inform us, that Pears do produce their Air, as it were by Paroxysms, or Fits.

ART I.

ARTICLE XI.

Various Experiments.

EXPERIMENT I.

March 16.

I Melted down Lead with a fire in a Brass Vessel, whose Diameter was an inch and half; but before the Lead was concreted by cold, I put it into a Receiver, out of which I exhausted the Air with great speed; whence it came to pass, that the figure of the concreted Lead, was concave, and the parts of it were so much the more depressed, by how much they were the nearer to the Center: whereas, on the other side, Lead congealed in common Air, doth exhibit a convex figure, except in the middle, where a little cavity doth appear.

I made the same Experiment with Tin, and had the same success: though both Metals being liquid, and very hot, had remained long enough *in vacuo*, yet no bubbles seemed to emerge from either of them; whereas all other hot liquors do send forth numerous bubbles *in vacuo*.

EXPERIMENT II.

September 2.

I put Water saturated with dissolved Salt, *in vacua*, to trie whether it would be there converted into Chrystals, and the Salt be carried above the plain, or superficies of the Water, as it is wont to happen in the Free Air.

Sept. 15. The Water with the dissolved Salt, abiding in the same

same state, I opened the Receiver; seeing no vapours could escape out of the evacuated Receiver, 'tis consentaneous to Reason to judge, that the Salt could not there be converted into Chrystals.

EXPERIMENT III.

August 8. 1676.

I put Air produced from Goosberries, into an evacuated Receiver, furnished with a Mercurial Gage.

March 1. 1674. When I perceived that no change was made in the height of the Mercury, I opened the Receiver.

EXPERIMENT IV.

August 8.

I took a Phial which was able to hold 7 ounces, 5 drams, and 3 grains of Water, and exhausted the Air out of it; and when in a ballance it was suspended in an *æquilibrium* with another weight, I pierced the bladder which covered the orifice, with a Needle, and then, the phial being filled with Air, appeared heavier by 4 grains and $\frac{1}{2}$, which latter weight to the former, is in the same proportion as 1 to 814; whence it follows, that Water is about 800 times more ponderous than that Air of an equal bulk. Yea, 'tis probable, that the proportion is with the least, because this day the Air was hot and clear, and besides some Air was always left in the Receivers after the exhaustion.

EXPERIMENT V.

Jan. 16. 1677.

I put *Aqua Fortis* with fixed Nitre into a Receiver, and, having exhausted the Air as much as I could, I poured in one of them on the other, and found much Air produced. I marked the height of the Mercury in the Gage.

March

March 5. Finding that the produced Air was not destroyed, and that the Mercury persisted in the same height, I opened the Receiver, and found Nitre produced *in vacuo* from the mixture.

E X P E R I M E N T VI.

May 12. 1677.

I filled a Phial, of a long and very narrow neck with Oil up to the middle of the neck; and thus filled, I put it into a Receiver firmly stopped by the help of a Screw; into which afterwards I intruded Air till it could sustain 120 digits of Mercury above its wonted height. And the Oil in the neck of the phial, appeared depressed toward the phial about one quarter of an inch; the cause whereof I judge attributable to the compression of the Air; and yet having eased the Screw, and thereby suffered the Air to break in and be dilated; the Oil did not ascend at all; so that I judge it was condensed onely by cold.

August 5. I made the same Experiment after the same manner, onely using Water instead of Oil; and yet I could perceive no change of the height of the Water in the neck of the Glass, though the heat being moderate, might have produced a sensible effect.

Jan. 14. 1678. Because I found by some Experiments, that compressed Air did enter into the pores of the Water, and did pierce even to the bottom, a suspicion might arise, that the Water was not condensed by the compressed Air, for this reason, because the Air entering into the pores, did make the pression within equal to the pression from without. And to be sure of this, I filled the Glass above said with Spirit of Wine, leaving onely the length of 3 digits in the top of the neck thereof, which was filled with Air onely. Then my hands being applied to the Glass, the Spirit of Wine, being heated, in a short time, filled the whole neck even to the top. Then the Glass being inverted into a Vessel

Vessel full of Mercury, I removed my hands, which being done, the Spirit of Wine being soon cooled; afforded space to the Mercury to fill 3 digits in height. I put the Vessel and the Glass in that posture, into a Receiver, into which I afterwards compressed the Air, till the Mercury exceeded its wonted height 90 digits, and yet there was no sensible condensation of the Spirit of Wine, nor any ascension of the Mercury; however it is certain, that no Air had crept in, because the Mercury hindered it; and the Receiver being opened, when the Air, that compressed without, was dilated, no bubbles appeared in the Spirit of Wine.

In this Experiment, it seems worthy our Enquiry, how it comes to pass that Spirit of Wine was so sensibly condensed by a moderate cold, and not at all by a great compression of the Air.

E X P E R I M E N T VII.

May 12. 1676.

I poured Spirit of Wine into a Glass Vessel, and superadded some drops of Oil of Turpentine thereto, which swimming upon the Spirit of Wine, began to be whirl'd about by motion, hither and thither, as it is wont to come to pass. I put the Glass Vessel on the Pneumatick Engine, and covered it with a Receiver, and yet the bubbles did not at all cease to be moved up and down. Then I pump'd out the Air, till the Spirit of Wine did onely not bubble; and it came to pass, that the bubbles emerging from the Spirit of Wine, did adhere to the drops of Oil, and carried them with themselves to the sides of the Vessel, and there retained them; yet 2 drops, free from such bubbles, proceeded to have further motion: Afterwards I wholly exhausted the Receiver, and some drops were emitted to the top thereof, by the force of the bullient Spirit of Wine; but the remaining drops proceeded on to be moved a little,

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and

and in a little time after they rested. The Air being immitted, the drops began again to renew their motion, but it was a slow one, and it quickly ceased.

I iterated the same Experiment, with Spirit of Wine and Oil of Turpentine, cleansed from Air; and no ebullition was then made, yea no bubble appeared at all, but the drops of the Oil of Turpentine were moved *in vacuo*, as in the open Air.

Hence it seems to follow, that the cause of the motion of the drops is not to be ascribed to the dissolution, for all the dissolutions *in vacuo*, have hitherto seemed to me to produce bubbles.

EXPERIMENT VIII.

May 19. 1676.

I left yesterday 2 Radishes *in vacuo*, one of them I hanged up, the root being upside down, the other in a contrary posture; both of them cut transversly did hang over a subjacent Vessel, which contained red Wine. All these being left a whole night *in vacuo* seemed well purged from their Air. Opening the Receiver, I added 2 other Radishes to the former included ones, cut after the same manner and from which I had further detracted their thick skin. Then exhausting the Receiver, I immersed the cut part of all the Radishes at once, into the subjacent Wine: and then many bubbles seemed to arise out from them, as it came to pass in those little Glas-Tubes of *Experiment IX*. yea more bubbles were emitted from those Radishes, which were purged from Air the whole night, than from those which had not remained above half an hour *in vacuo*; and from whom I had taken away their skin.

This Experiment seems to afford us a confirmation, that Bubbles are formed of particles of Air, swimming in Water; and because in the skin there are some Canales, fit to retain parts of Air, it came to pass that the Radishes, from which I had detracted their skin, afforded no opportunity for the forming of so many Bubbles.

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The liquor ascended no less into those Radishes which hanged with their roots upwards, than into those of a contrary posture.

EXPERIMENT IX.

May 4. 1676.

I immersed one end of a small Glass tube, open at both ends, into Water stagnant *in vacuo*, and presently the Water ascended up into it, as it is wont to do in common Air, and even to the same height; but a little while after, many Bubbles being formed there, lifted the Water higher, and kept it suspended in 3 different places, disterninated by many Bubbles; and many other Bubbles seemed to pass out from that end, which was immersed in Water.

Then I sealed the other end of the tube Hermetically; and so the Experiment being made in common Air, the Water could not ascend up into the tube by the open end. But *in vacuo* the matter succeeded far otherwise; for the Water ascended up into the tube, no otherwise, than if it had been open at both ends; and many Bubbles formed in a short time, did distinguish the Water, contained in the tube, by great intervals, as before, whilst the mean time, many other Bubbles seemed incessantly to pass out from the end of the tube, immersed in Water, yet in progress of time, they appeared less frequent.

But this circumstance I much admired, that the Water being suspended higher in the tube, seemed to be filled with no Bubbles, whereas the end onely did emit so many.

Then I took out that end from the Water, and no Bubbles did any more appear, though that end was wholly filled with a Cylinder of Water.

May 5. I repeated the same Experiment; but before I had immersed the end of the tube in Water, a drop of Water which ran over from the superiour aperture of the Receiver, flowed

down to the open end of the tube, and pierced up into it the height of 2 lines, neither was any Bubble formed there in a full half hours time: that being passed, I immitted the end of the tube into the Water of the Vessel, and not long after, Bubbles began to be formed, as before, of which some followed others within half a minute; yet afterwards they came forth less frequent. Furthermore, iterating this Experiment many times, I perceived, that when the Water was extracted from the tube, no Bubbles appeared: but if it were immersed in Water, Bubbles would cleave to the end of it, either sooner or later.

May 6. I tried the same Experiment, with the infusion of Nephritick-wood, and the success was wholly alike, but that the Bubbles could emerge and pierce the liquor, before they had acquired any bigness, for being yet very small, they pervaded the liquor, contained in the tube, and were carried to the upper part thereof: whence we may conjecture, that that liquor is very thin, and hath no viscosity to resist the pervading Body.

May 10. I iterated the same Experiment with Spirit of Wine, mixed with a certain Oil, made *per deliquium*: yet I found no new event, but that the ascension of the liquor into the tube, was not so high.

From these Experiments it seems to follow, that the Bubbles are formed, in the extremity of the tube of aerial particles, swimming in the Water, which finding some impediment at that end, cannot pass by, and so, new ones coming upon them, they swell into a Bubble.

EXPERIMENT X.

July 18. 1676.

Two days ago I took some Beans, such as are given to Horses for Provender, and included them in an iron tube closely stopped; yet I first affused Water on the compressed Beans, till the

the tube seemed wholly full; to try whether the force of the swelling Beans would be enough to break the tube. This day the tube seemed not to be altered at all, but the stopple being plucked back, some quantity of Air brake out; and much Water fell upon the ground, which was not sucked up by the beans; then a certain noise, as it were, of bubbling Water, was heard for a whole hour and more.

July 25. I left the iron tube in the same posture, but this day one of the ends of it being unstopped, and some Beans taken out, the murmur of the bullient Water was heard, as before.

From this Experiment it seems to follow, that Beans do contain Air in them, which in a great compression cannot escape out; but if it be freed from the force compressing it, then it makes an eruption.

EXPERIMENT XI.

March 4. 1677.

I put a Glafs half full of Spirit of Sal-Armoniack and *limatura Cupri*, into a Receiver exhausted as much as I could, and there stopped it in. And it came to pass, that in 15 minutes space the liquor had contracted a certain blew colour, but very much diluted; but, the Air being immitted, in 3 minutes, the blew colour appeared vivid and thick. I put the liquor so tinged again *in vacuo*, to trie, whether in tract of time that colour would vanish.

April 4. The blew colour was almost quite vanished, but upon the admission of the Air, it quickly returned.

EXPERIMENT XII.

May 8.

I put a certain Oil made *per deliquium*, with Spirit of Wine
into

into an exhausted Receiver, and the Spirit always swam on the top; now lest the Spirit might be spilt by bubbling above the edges of the Vessel, I extracted the Air by degrees, and in the beginning great Bubbles came from the Spirit, and but very small ones from the Oil; but after one hours time, the Oil did emit great Bubbles, which being small at bottom, in their ascent did fill the whole latitude of their Vessel; and after another hour, some Bubbles brake out with so great force, that they hit against the top of the Receiver.

May 9. I iterated the former Experiment in a Glas somewhat long and narrow, that I might the better perceive the motion of the Bubbles; and then I saw the Bubbles passing out of the Oil into the Spirit of Wine, without any great increase of their quantity; but being distant onely 1 quarter of an inch from the superficies, they were suddenly expanded.

EXPERIMENT XIII.

May 3. 1676.

I mixed a certain quantity of *Aqua Fortis* with a quantity of *Spirit of Wine* somewhat greater; and then I distributed that mixture equally into 3 Glas Vessels, and put three equal pieces of Iron into them, to each Vessel one. This being done, I included one of the 3 Vessels *in vacuo*, and there many great ebullitions were made. Then after a quarter of an hour, I took out the Vessel, and found the liquor black and turbid, whereas the other two Vessels had their liquor not altered in colour, but onely some black powder did appear in the bottom of the liquor.

Of these 2 Vessels, I put one *in vacuo*, and then there arose ebullitions, great indeed, but much lesser than the former: when one quarter of an hour was elapsed, I took the Vessel *è vacuo*, and found the liquor black indeed, yet somewhat less so than the former; but the liquor which was left always in the Air, did in a manner remain unchanged

May

May 4. This day in the morning the liquors in the 2 Vessels, put *in vacuo*, appeared cleansed and green, and had no other operation.

But the liquor which was not put *in vacuo* did bubble more strongly than yesterday, and exhibited a red colour. I put the 3 Vessels together *in vacuo*, and perceived no eminent ebullition, only some Bubbles appeared larger in the red liquor, than in the other two.

From this Experiment it seems to follow, that Spirit of Wine *in vacuo* doth accelerate ebullition.

EXPERIMENT XIV.

Jan. 21. 1678.

I kept a Glas half full of Sal Armoniack, and *filings of Copper*, the hole thereof being so exactly stopped, that the blew colour, which was induced into that liquor, from the contact of the external Air, (*See Philosophical Transactions, Num. 120.*) did wholly now disappear. The stopple was made of Leather, prepared after a special way and manner.

I put that Glas *in vacuo* with Paste not yet fermented.

I did it to this end, that the Receiver, being full of Air from the Paste, I might perforate the leather that stopped the Glas, with an Iron Wire prepared for that purpose; and that I might trie, whether the contact of the Air generated from the Paste, would also communicate some colour unto the liquor.

Jan. 22. There was no need to perforate the Leather, for this day I found the liquor already tinged; so that it is probable, that Air produced from Paste, is endued with such minute particles, that it can penetrate Leather which is impervious to common Air.

Yet I will keep the Glas, not touching its ligature, to trie, whether that colour may vanish again.

Jan. 25. Now the liquor became almost colourless, whence
it

it appears, that common Air is too thick to penetrate all passages, which are pervious to Air, produced from Paste.

Feb. 2. I put the same phial *in vacuo*, but did not fortifie the commissure of the Receiver with the Cover, with Turpentine, so that the Air making a gradual ingress, in 24 hours filled the Receiver, even as it was leisureably filled, with the Air produced from Paste, yet the liquor remained still colourless.

Feb. 15. I put the same Glass again *in vacuo* with some quantity of Paste; but this time the Air produced from thence, did not pervade the Leather, as it had done before, and the liquor was not tinged at all.

EXPERIMENT XV.

April 2. 1678.

I put a Shrew-mouse into the Engine described p. 13, 14. and when I perceived he was reduced to extremity, I began to stir the Pump, that the Air might penetrate, and be, as it were filtrated through the Water. The Mouse a while after, seemed to be better, yet he could not be wholly restored to health. Now because he had been long kept fasting, I am uncertain whether he died for want of Aliment, or of new Air.

April 12. I iterated the same Experiment with a small and weakly Mouse, that had been kept a long time fasting. And finding that this Experiment had the same success with the former, I took out the Mouse, before he was dead; and though he then enjoyed the Free Air, yet he recovered not; so that we have need of more Experiments, that we may attain to a certain knowledge of the effect of that Filtration.

EXPERIMENT XVI.

May 2. 1678.

Six Weeks ago, I included Frog-Spawn in 3 Recipients;
the

the first of which was *vacuous*; the second contained common Air; and into the third, I intruded so much Air, that the Mercury staid in 60 digits above its wonted height.

In 15 days the Mercury in the evacuated Receiver came to the height of 1 digit. The Spawn in the common Air seemed corrupted and of a blackish colour; but that in the compressed Air, remained unaltered in colour; but no Frogs were generated.

After an whole month was elapsed, the Sperm *in vacuo* had not changed its colour, excepting the black round spots, but seemed reduced into Water: the colour of that in the common Air was very black, but in the compressed Air the Spawn began to be reddish.

As yet no change was perceived, neither in that Spawn *in vacuo*, nor that in the common Air; but in the compressed Air, the Spawn waxed more and more red.

May 22. The Sperm *in vacuo* was not changed; in the compressed Air it remained red; but in the common Air it became again colourless.

June 23. The Sperm *in vacuo* and in common Air was tinged with no colour, but in the compressed Air it inclined to greenness.

Octob. 15. I took out all the Spawns; that which was kept *in vacuo* was almost exhaled out of its Vessel, and was stagnant in the Receiver, like clear Water: In the common Air, the Sperm remained colourless; but that in the compressed Air kept still its red colour.

EXPERIMENT XVII.

May 9. 1678.

Six days ago, I included two pieces of the same Orange in 2 Receivers, not quite of equal bigness, but in the greater Receiver, there was left some quantity of Water, so that no less space

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was

was left for the Air in that, than in the lesser. The issue was, that the Orange included with Water, though it were not touched by it, yet was 4 times more mouldy than that which was kept without Water.

And therefore in iterating this Experiment, I put 2 pieces of the same Orange into 2 Receivers, but I filled the third part of one of them with Water, yet so, that it did not reach the Orange.

June 15. Neither of the Oranges had contracted any mouldiness.

May 16. I repeated the same Experiment with the same success, yet neither Orange had acquired any mouldiness in the space of more than a month, though in former Experiments all such Oranges would be mouldy.

The cause of the difference seems to be attributable to some disposition of the Air.

EXPERIMENT XVIII.

June 1. 1678.

I put a small Glas-tube, half full of Venice Turpentine, into the Wind-gun, described p. 16. and I had scarce reduced the Air to the tenth part of its wonted space, but the Leather, spread over the Elliptick valve, was driven out; so that the Air having made an escape, I drew the Glas-tube out of the Engine, and found many Bubbles formed in the superficies of the Turpentine; and therefore I suspected that the Air had pervaded the Turpentine, and that it would have penetrated more deeply into it, if they had remained longer thus enclosed together: and therefore I re-immitted the same Tube into the same Gun, and there left it in Air reduced to about the 15 part of its space.

June 3. I opened the Engine, and, taking out the Tube, found the Turpentine almost free from Bubbles, yet by degrees many
were

were formed therein, in the parts remote enough from the superficies.

June 4. I threw away the former Turpentine, and put new in the same Tube, and included it *in vacuo*, that the Turpentine might be the better purged from all Air; then I poured Water upon it, and shut up all in the Wind-gun.

June 8. I opened the Engine, and at first sight, both the Water and the Turpentine in the Tube, seemed to be very free from Bubbles; but a little while after I perceived, that Bubbles were formed in the Turpentine, and that they ascended by degrees; yea, some of them seemed to be made almost in the very bottom, about half an inch below the superficies of the Turpentine. So that we may conjecture, that all the Water, and so great an height of the Turpentine, were penetrated by the Air, which formed those Bubbles.

EXPERIMENT XIX.

August 11. 1678.

I included Spirit of Sal Armoniack, with a Mercurial Gage *in vacuo*; and after that the Spirit ceased to emit any Bubbles, I mixed Filings of Copper therewith, which caused many Bubbles to break forth again; but they were so far from producing any Air, that they contrariwise consumed that which was there before. As it hath been already observed in the Philosophical Transactions, N. 120. But the liquor was made greenish and turbid.

Decemb. 5. The Spirit was almost all exhaled out of the Vessel, in which it was contained, and being condensed in the Receiver, remained still turbid, by reason of much filth which was included there: but that which was not exhaled out of the Vessel, appeared clear like Water. Also the Mercury was wholly expelled out of the Gage. Whence I conjecture, that the Air in the Receiver, was more and more consumed.

EXPERIMENT XX.

September 2. 1678.

I put 2 Cylinders, one of Tin, the other of Lead, *in vacuo*; but their lowest parts were immerfed in Mercury; and at the fame time I immerfed 2 other Cylinders, like the former, after the fame manner in Mercury: but thefe latter were left in the free Air.

Sept. 6. I opened the *vacuous* Receiver, and the Mercury in the Tin Cylinder, was come to the height of 4 digits and an half above the superficies of the stagnant Mercury; and cutting the Cylinder tranfverfly, in the middle of that height, the Amalgama feemed to have penetrated into the Cylinder, about half a line. And cutting the Cylinder tranfverfly again, in that part which was diftant onely 1 digit, from the superficies of the stagnant Mercury, I found the thicknefs of the Amalgama to equal one line.

In the Lead Cylinder the Mercury came to the height of 2 digits and $\frac{1}{2}$, but only as far as the superficies, and that very part which was immerfed in the Mercury, was not penetrated by it to any fenfible thicknefs.

Sept. 7. I took out the Tin, left in the Air, out of the Mercury in which it was immerged, and found the Mercury to have afcended to the height of 5 digits.

Sept. 10. The fame Cylinder being left in the Mercury, feemed to be befmeared therewith up to the very top, 6 digits and more, above the superficies of the stagnant Mercury. When the Cylinder was tranfverfly cut in feveral places, it appeared that the Mercury had pierced fo much the higher into the Tin, by how much it came nearer to the stagnant Mercury; fo that in the part near to the forefaid Mercury, almoft the whole diameter of the Cylinder, 3 lines broad was penetrated thereby.

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In the Lead Cylinder, the Mercury exceeded not the height of 3 digits and $\frac{1}{2}$, neither had it penetrated to any sensible thickness. Whence it appears, that the weight of the Air, contributes little or nothing to the ascension of Mercury into Metals.

EXPERIMENT XXI.

Decemb. 12. 1678.

I took a small Whiting, and having cut off his head, I divided him transversely into 5 pieces. The first whereof I included *in vacuo*. The second in common Air. The third in Air so compressed, that it could sustain Mercury 50 digits above its wonted height. These 3 Receivers were closed with Screws. The fourth piece was put into a Receiver, full of Air produced from Paste, which was presently stopped. The fifth was left in the Free Air.

Decemb. 15. This day in the Morning, that part of the Whiting, which was left in the free Air, began to shine; and towards Evening it sent forth somewhat a more vivid light.

Decemb. 16. In the Morning, the Whiting left in the Free Air, gave over shining; but towards Evening it shone again.

Decemb. 17. This Morning the same part of the Whiting shined a little, yet less than it did yesterday in the Evening.

Decemb. 18. In the Morning there appeared no light, though I fixed my eyes a long time upon the Receiver in a dark place; but the Night coming on, the light appeared again.

Decemb. 20. Hitherto the same part of the Whiting left in the Air, proceeded to shine; but all the other parts did not yet begin to shine.

Decemb. 22. Yesterday the light of the Whiting left in the Air, had not quite ceased, but this day it appeared no more.

Decemb. 24. The part of the Whiting in the Free Air, gave over its shining quite; but that which was included with com-
mon

mon Air, did yesterday send forth a faint light; but this day it proceeded not to shine.

Decemb. 26. No shining appeared any more in the common Air: but the three other pieces did not so much as begin to shine.

Jan. 26. 1679. I perceived no more shining in any one of the Receivers.

A R T I C L E XII.

Artificial Air destroyed.

E X P E R I M E N T I.

August 3. 1677.

I Transmitted Air produced from Cherries, into a Receiver full of Common Air, but so stopped with a Screw, that the Mercury ascended to 25 digits above its wonted pressure.

Aug. 4. The Mercury was depressed about 2 digits. The height of it this day was only 23 digits.

Aug. 6. The height thereof was reduced to 20 digits.

Aug. 7. The height thereof was the same.

Aug. 8. The Mercury was somewhat depressed.

Aug. 10. The height of it was $19\frac{1}{2}$ above its wonted height. When I perceived little or no alteration more, I opened the Receiver.

From this Experiment we have a confirmation, that Air produced from Fruits, at the beginning is in part destroyed; but the rest can keep the form of Air very long.

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EXPERIMENT II.

May 26. 1676.

I took 6 grains of Sal Armoniack, and put them into a Receiver, with a sufficient quantity of Oil of Vitriol: then the Air being exhausted, I forced down the Salt into the Oil; whereupon a great ebullition presently followed, and the Mercury ascended into the Gage, almost to its wonted height; but presently after it sunk again, and returned to its former state.

May 27. I repeated the same Experiment, but this time the Salt remained 10 hours *in vacuo*, before it was put into the Oil; but the ebullition followed, as in the former Experiment; yet the Air was produced much more slowly, neither could it wholly be destroyed, but in 7 or 8 hours time; yet at last the Mercury descended to the very bottom.

May 29. I tried the same Experiment again, leaving the Materials 24 hours *in vacuo*: This time the ebullition seemed much less, and the Air was produced both in a lesser quantity, and more slowly than before. I observed also, that whilst the Materials staid *in vacuo*, before their mixture, that the Mercury came nearer to the open end of the Gage, as if some Air had been either extracted or destroyed.

July 8. I put Oil of Vitriol alone into a Receiver, in which I left onely a fifth part of common Air, to trie whether this Oil, without Sal Armoniack, would diminish the elastical force of the Air: but it fell out contrary, that the force of the Air was increased, and the Mercury in one hours space, seemed to have ascended a little into the Gage; but afterwards for 24 hours space no change was made.

This Experiment doth confirm, that some Artificial Airs, may be destroyed; but why this destruction happens sometimes sooner, sometimes slower, it may perhaps seem worthy of a further enquiry.

ARTI-

ARTICLE XIII.

Experiments concerning the different celerity of Air produced in vacuo, or in common Air.

EXPERIMENT. I.

COMMON AIR.

July 10. 1676.

I Put Paste, kneaded two days before, and sowerish, into a Receiver, and stopped it firmly with a Screw.
In one hours space the height of the Mercury was one digit.

In 7 hours space the height of it was 6 digits.

July 11. The height of it was 11 digits.

12. The height of the Mercury was 24.

13. The height thereof was 30.

14. The height of the Mercury was sensibly greater.

15. The Mercury ascended a little. Measuring its height exactly this day, I found it 38 digits.

19. No more Air was produced from the Paste.

VACUUM.

July 10. 1676.

I put another quantity of the same Paste, much less than the former, into an exhausted Receiver.

Though the quantity of the Paste was less, yet in one hours time, the height of the Mercury was 2 digits.

In

In 7 hours time, the Mercury came almost to the top of the Gage, but it was a short one.

July 19. The Paste was not able to remove the Receiver from his Cover, though at the beginning it had produced a greater quantity of Air than the Paste in common Air. I endeavoured to fire it with a burning glass, and the Fumes, elevated therefrom, afterward falling upon the Paste, did tinge the superficies thereof, with a pleasant yellow colour; and that Air was thus produced, I conjectured hereby, because the Cover was afterwards easily severed from its Receiver.

From this Experiment made in 2 Receivers at once, we learn, that Air is sometimes generated much more easily *in vacuo* than in common Air.

EXPERIMENT II.

COMMON AIR.

August 20. 1676.

I put Paste, kept for 24 hours, into a Receiver full of common Air; to which I further added new Air, so that the Mercury exceeded its wonted height 4 digits and $\frac{1}{2}$.

In 6 hours space the Mercury gained almost 4 digits. Its height was 8 digits.

August 21. The ascension of the Mercury was 4 digits and $\frac{1}{2}$.

Aug. 22. The ascent of it was about 1 digit.

23. The ascent of it was $\frac{1}{2}$ a digit.

26. For 3 whole days the ascent of the Mercury was onely $\frac{1}{2}$ a digit.

27. There was no ascent of it at all.

29. The Paste taken out of the Receiver, affected our Nostrils with an acid smell.

X

VA-

V A C U U M.

August 20. I put another quantity of the same Paste into an empty Receiver, and kept the same proportion between the quantity of the Paste and the capacity of the Vessel, as in the former Experiment.

The Mercury seemed to have ascended in a short time. Its height was 2 digits.

Aug. 21. The ascent of the Mercury was 5 digits.

22. The ascent of it was 3 digits.

23. The ascent of the Mercury was 1 digit.

26. For three whole days the ascent of it was 2 digits.

27. There was no ascent of the Mercury.

28. I took out the Paste exhausted of its Air, from the Receiver.

This Experiment confirms to us, that Air is sometimes more easily produced *in vacuo*, than in common Air.

E X P E R I M E N T III.

V A C U U M.

Septemb. 4. 1677.

I put the Kernels of Filberds into an exhausted Receiver.

Sept. 5. The height of the Mercury was 5 digits.

Sept. 6	} The height of it was	10	} Sept. 11.	} The height of it was	18	
7		10			12	23
8		12			13	27
9		15			14	29

Sept. 15. The height of it was almost the same.

17. The height of it was 30.

18. This day the Air began to escape out of the Receiver, for some Bubbles appeared in the Turpentine, which strengthened the Commissure of the Receiver and Cover.

COM-

COMMON AIR.

September 4.

I put Kernels of Filberds into a Receiver with Common Air.

In the Afternoon the quantity of Air seemed to be lessened.

Sept. 5. The height of the Mercury, was less than half a digit.

6. The height of it was the same.

7. The height of it was 1 digit.

8. The same height still continued.

18. The same height continued.

This Experiment gives us a confirmation, that sometimes Air is produced much more easily *in vacuo* than in Common Air.

EXPERIMENT IV.

September 15. 1677.

I included 8 ounces of Raisins of the Sun, bruised and diluted with a little Water, in an exhausted Receiver, able to hold 22 ounces of Water.

Sept. 16. The height of the Mercury was 6 digits.

Sept. 17 } The height { 10	Sept. 19 } The height { 29
18 } of it was { 15	20 } of it was { 29

Sept. 21. This day I found the Receiver forced from his Cover.

Sept. 24. I took out some of the Raisins; but those that remained, I enclosed in the same evacuated Receiver.

Sept. 25. The Raisins forced the Receiver, now full of Air, from his Cover.

September 15. 1677.

I put 8 ounces of Raisins of the Sun, bruised and diluted
X 2 with

with a little Water, into a Receiver, able to hold 22 ounces of of Water ; but I did not exhaust the Air at all.

Sept. 16. The height of the Mercury was $\frac{1}{4}$ of a digit above what was accustomed.

Sept. 17. The height of the Mercury was $1\frac{1}{2}$.

18. The height of it was 3.

<i>Sept.</i> 19	} The height of it was	{ 5 7 9	<i>Sept.</i> 22	} The height of it was	{ 11 12 15
20			23		
21			24		

Being about to put Peaches into the Receiver, I permitted the Air to break forth; and then many Bubbles did emerge from the Raisins.

This Experiment doth further teach, that Air is sometimes much more easily produced *in vacuo* than in common Air.

EXPERIMENT V.

VACUUM.

February 17. 1677.

I put 3 Onions into an emptied Receiver.

Febr. 19. The ascension of the Mercury was 1 digit.

21. The ascent thereof was again 1 digit. The Onions were not altered.

25. The whole ascent of the Mercury was 9 digits
The Onions not altered.

May 4. The Onions had yet undergone no alteration.

18. Neither were they yet altered.

June 19. I this day found the Receiver, forced from his Cover, and the Onions rotten.

RARIFIED AIR.

Febr. 17. I inclosed 3 Onions in Air so rarified, that it could sustain onely 10 digits of Mercury.

Feb.

- Feb. 19. There was no ascent of the Mercury.
 21. There was yet no ascent thereof. The Onions did not germinate, but contracted a mouldiness.
 25. The ascension of the Mercury was about 7 digits. The Onions received no further alteration.
 May 4. The Onions were not altered.
 18. The Onions were not yet altered, but the Receiver, by the force of the produced Air, was removed from his Cover.

C O M M O N A I R .

February 17. I put three Onions in a Receiver not exactly shut.

21. The Onions contracted no mouldiness, but did germinate.

25. The Onions put forth root more and more.

May 4. The Onions began to be mouldy.

This Experiment gives us a likely proof, that *some* Bodies do produce their Air not much more easily *in vacuo*, than in rarefied Air.

And besides it hereby appeareth, that Vegetation is hindered, not onely by the evacuation, but also by the rarefaction of the Air.

It seems also worthy our observation, that the Onions, as long as they emitted roots, did contract no mouldiness.

ARTI-

ARTICLE XIV.

*The difference betwixt whole, or entire, and
bruised Fruits.*

EXPERIMENT I.

BRUISED FRUITS.

August 23. 1677.

I Put Pears bruised into a vacuous Receiver, with a Mercurial Gage.

August 25. The height of the Mercury, was 5 digits.

<i>Aug. 26</i>	} The height { 10	<i>Aug. 29</i>	} The height { 21	
27		30		25
28		31		28
} of it was { 14		} of it was { 25		
} of it was { 18		} of it was { 28		

Sept. 1. The height of it was 30.

2. The Receiver was found forced from his Cover.

WHOLE, OR ENTIRE FRUITS.

August 23. I put whole Pears into a *vacuous* Receiver, and I took care that the quantity of the Pears, and the capacity of the Receiver, might be the same with those which I mentioned before.

Aug. 25. The height of the Mercury was 11.

<i>Aug. 26</i>	} The height { 17	<i>Aug. 28</i>	} The height { 28
27		29	
} of it was { 25		} of it was { 30	

Aug. 30. The Mercury ascended no higher, because the Receiver was forced from his Cover.

This

This Experiment seems to prove, that Bruised Fruits do not produce air as soon as Entire ones.

EXPERIMENT II.

ENTIRE FRUITS.

August 24.

I enclosed whole Apples *in vacuo* with a mercurial Gage.

August 25. The height of the Mercury was 5 digits.

Aug. 26	} The height {	9	} Aug. 29	} The height {	19	
27		12			30	25
28		15			31	28

of it was

September 1. The height of it was 29.

2. The height of it was 30.

3. The Receiver was forced from his Cover.

BRUISED FRUITS.

August 24. I put an equal quantity of bruised Apples into a vacuated Receiver, of the same capacity with the former.

Aug. 25. The height of the Mercury was 1 digit.

26. The height of it was 3 digits.

27. The height of it was 4.

Sept. 3. The Mercury continued in the same height.

25. The Mercury ascended not at all.

This Experiment seems to inform us, that *bruised* Fruits do produce air, slower than *whole* or entire ones.

EXPERIMENT III.

BRUISED FRUITS.

Aug. 25. 1677. I put unripe Grapes bruised, into a vacuated Recipient.

Aug.

- Aug.* 26. The height of the Mercury was 1 digit.
 27. The height of it was 2 digits.
 28. The height of it was 2 digits and an half.
 29. The height of the Mercury was the same.
Sept. 15. The Mercury did not ascend at all, but its height remained at $2\frac{1}{2}$.

W H O L E F R U I T S .

August 25. 1677. I put unripe Grapes, not bruised, into a vacuated Receiver.

- Aug.* 26. The height of the Mercury was 3 digits.
 27. The height of the Mercury was 5 digits.
Aug. 28 } The height { 7 | *Aug.* 30 } The height { 12
 29 } of it was { 10 | 31 } of it was { 13
Sept. 1. The height of the Mercury was 15.
 2. The height of it was 16.
 3. The height of it was 18.
 4. The height of it was the same.
Sept. 5. The height of the Mercury continued the same; but all the Grapes had almost contracted a yellow colour.
Sept. 7. The Mercury rested in the same height; but all the Grapes were yellow.
Sept. 15. The height of the Mercury was 20.
 This Experiment gives us a further confirmation, that *whole* Fruits do produce air, more readily then bruised ones.

E X P E R I M E N T I V .

F R U I T S W H O L E A N D E N T I R E .

September 10. 1677.

I put 2 ounces of ripe Grapes, but not bruised, into a Receiver able to hold 10 ounces of Water.

Sept.

Sept. 11. The height of the Mercury was 6 digits.

Sept. 12	} The height	{ 9		Sept. 15	} The height	{ 20	
13				{ 12			{ 16
14							
} of it was		} of it was					

Sept. 18. The height of the Mercury was 30. The Grapes were not altered at all.

Sept. 19. The height of the Mercury was the same.

20. The Receiver was not yet forced from his Cover. The Grapes were not altered, but appeared onely a littleriper.

21. The Receiver was forced from his Cover, though as yet nothing had made any eruption out.

22. This day in the Morning, I found the Grapes begin to rot, and therefore I included them again *in vacuo*.

Sept. 23. The height of the Mercury was 5 digits.

Sept. 24	} The height	{ 9		Sept. 27	} The height	{ 20	
25				{ 14			{ 29
26							
} of it was		} of it was					

Octob. 10. The Receiver was not forced from his Cover, till this day: the Grapes by their colour seemed rotten, yet they had kept their firmness.

B R U I S E D F R U I T S.

Sept. 10. 1677. I included two ounces of ripe and bruised Grapes in a Receiver capable of holding 10 ounces of Water.

Sept. 11	} The height	{ 4		Sept. 15	} The height	{ 15			
12				{ 7			{ 16		
13								{ 10	{ 17
14									
} of it was		} of it was							

Sept. 19. The Grapes had severed the Receiver from his Cover, and much juice was spilt.

Sept. 20. I again put the same Grapes into the same Receiver; but because they had spilt their juice by ebullition, I did

Y

not

not exhaust all the Air, but the Mercury staid in the height of 5 digits.

Sept. 21. This day in the Morning, the Receiver, being now full of Air, did no longer stick to his Cover; so that I took out the Grapes, and transmitted them into another Receiver, which I stopped close with a Screw, but extracted no Air from it.

Sept. 22. The height of the Mercury was 11 digits, though the Receiver was able to hold 26 ounces of Water.

Sept. 23. The height of the Mercury was 19.

24. The height of it was the same.

30. The height of it was 20.

Octob. 3. When the Grapes produced no more Air, I took them out, and found them of a bitter taste, because they were not yet come to their perfect ripeness.

This Experiment, if you compare it with *that*, which I related before concerning unripe Grapes, doth seem to intimate, that unripe Grapes do produce less Air when they are bruised, than when unbruised; but ripe Grapes do the contrary.

EXPERIMENT V.

Nov. 19. 1678.

I put Apples into 3 vacuated Receivers. In the first was a sound Apple; in the second, an Apple bruised, and repositied loosely in the open Vessel: In the third was also a bruised Apple, and repositied in the Vessel, but the Cover was so fitted to the Vessel, that it did straitly compress the parts of the Apple. For I was desirous to know, whether the *bruised* Apple would produce Air *in vacuo*, as well as the *sound* one, provided his parts were narrowly conjoined; but the issue was, that in the exhausting of the Receiver, the Air, formed between the parts of the Apple, did expel all the juice.

Nov. 21. In the first Receiver the height of the Mercury was

was 5 digits; in the second, 3 digits; in the third, none at all.

Nov. 23. In the first Receiver the height of the Mercury was 7: in the two others there was no change.

Decemb. 7. In the first Receiver the height of the Mercury was 11 digits. There was no alteration in the other two.

Jan. 23. The first Receiver was now severed from his Cover, by the force of the Air produced anew. In the two others there was no Air generated.

May 20. 1679. This day the third Receiver was found forced from his Cover: whereas the second had produced no Air.

This Experiment informs us, that *bruised* Fruits do produce less Air *in vacuo*, than *sound* ones; contrary to what happens in common Air. The reason whereof may perhaps be this, that Fruits bruised are very much rarefied *in vacuo*, and so the several principles, of which they consist, cannot act upon one another: but unbruised Fruits, by reason of the entireness of their ambient skin, undergo less rarefaction.

ARTICLE XV.

Air is sometimes found unfit to produce mouldiness.

EXPERIMENT. I.

July 12. 1678.

I Put Roses into two Receivers, which were to be stopped with Screws. One of them contained common Air uncompresed; but I intruded so much Air into the other, as sustained the Mercury 60 digits above its wonted height.

Y 2

August

August 2. The Roses in the common Air, 4 days ago; were turned into a yellow colour, as if they had been withered: but those in the compressed Air kept their colour very well.

Febr. 10. 1679. The Roses in the compressed Air, as yet retained their fresh colour.

This Experiment, compared with that which was made the Year before with Roses, doth inform us, that the Air at divers times is diversly affected; so that sometimes it hath a power to hinder corruption, and sometimes to promote it. See *Artic. IV. Exper. IV.*

EXPERIMENT II.

May 22.

Fifteen days ago I included two equal quantities of Flowers, in two Receivers: Into one of them I thrust so much Air as sustained the Mercury 60 digits above its wonted height; but in the other, I left common Air incompresssed. The Flowers were Tulips and Larkspurs.

Since that time no mouldiness appeared, except onely that 10 days ago, one half of a Tulip, being cut in two, in the common Air, seemed somewhat mouldy: but this day, the other half of the same Tulip in compressed Air, seemed to be infected with some mouldiness.

As for the Flowers, some of them seemed as fresh, as when they were first put in; especially those in the common Air; for in the compressed Air, they seemed more moist.

June 22. No more mouldiness appeared: whence we have a confirmation of the Inference drawn from the former Experiment, *viz.* That the Air is sometimes unfit to produce mouldiness; seeing the year before, all those kind of Flowers had contracted a great deal of mouldiness.

ARTI

ARTICLE XVI.

Experiments concerning the change of weight, made by the Beams of the Sun, even in Vessels sealed Hermetically.

EXPERIMENT I.

Sept. 4. 1678.

I Exposed one drachm of Minium, in an open Glas to the Sun Beams concentrated in a Burning glas, and I found that it had lost $\frac{1}{4}$ of a grain of its weight, though much of the *Minium* had not been touched by the Solar-rays.

EXPERIMENT II.

September 6.

I took Coral, already calcined in fire, and endeavoured to calcine it further by the Beams of the Sun, in a sealed Glas; but I could scarce produce any good effect; yet the whiteness of the *calx* of the Coral was somewhat increased.

Sept. 10. I exposed the same Coral again to the Sun Beams in the same Glas Hermetically sealed, for two whole hours; and weighing the Glas: found that the loss of its weight, was about $\frac{1}{2}$ part of a grain, since the time it was first sealed.

EXPERIMENT III.

May 23.

I put *Calx* of Tin in a light glas phial, sealed Hermetically

cally, and weighed it exactly : afterwards I exposed it to the Beams of the Sun for a long time, by the help of a large Burning-glass ; then the Glass, being again weighed, seemed to have lost $\frac{1}{24}$ part of a grain of its weight.

May 29. I repeated the same Experiment, onely using *Minium* in stead of *Calx* of Tin, and the loss of weight came to $\frac{1}{12}$ part of a grain.

May 30. I endeavoured to burn the same *Minium* again, but such plenty of Air was produced, that the Glass broke into an hundred pieces, and made a great noise at its dissolution.

June 6. I tried the same Experiment again with *Minium*, and then $\frac{1}{24}$ part of a grain was abated of the weight.

When I attempted again to burn the *Minium*, the Glass broke a second time.

July 15. I took Coals made of Wood for the same Experiment, but the Sun did not affect them at all.

July 20. I exposed *Vive Sulphur* to the Beams of the Sun, after the manner before described; and though it was easily melted, and did emit many fumes, yet I found no change at all in the weight.

Aug. 1. I kept the same phial still with the Flower of Sulphur, and exposed it often to the fire of my Burning-glass, without danger of being broken, *viz.* because Sulphur produceth no Air ; but the Fumes were emitted, as at the first, and the Sulphur bubbled up ; but the weight seemed not to be changed.

A R T I C L E X V I I .

The Preservation of Bodies in compressed Liquors.

E X P E R I M E N T I .

August 3. 1678.

I Included two Apricocks in two Receivers, one of which was exactly filled with Raisins of the Sun bruised, and with Water; but in the other, there were onely some Raisins enclosed, yet so that the Apricock was not touched, neither by the Raisins, nor by the Water.

Sept. 10. I took out the Apricock, inclosed with the Water; and whilest the Air did break forth, the Fruit did bubble very much: the Raisins had lost almost all their taste, but the Apricock had preserved a pleasant relish; yea, it seemed more pleasant than the taste of such Fruits bought at that time of the Year useth to be.

Feb. 10. 1678. The Apricock, inclosed without Water, as yet kept its colour and figure, onely seemed to have lost its firmness.

This Experiment informs us, that the taste of some Fruits may be preserved in an Infusion of Raisins of the Sun; at least in Vessels which are able to contain a great compression of the Air.

E X P E R I M E N T I I .

Sept. 17. 1678.

I included Peaches, with an Infusion of Raisins, in 2 Receivers, shut with a Screw.

Sept.

Sept. 21. Too great a quantity of Air produced in one of my Receivers, expelled some part of the liquor out of it. The other Receiver as yet retained its liquor.

Sept. 25. The Receiver, out of which the liquor was expelled, lost some more thereof, so that its fifth or sixth part now seemed empty : but *setting* the Screw, the liquor was then preserved. The other Receiver was not altered.

Sept. 26. The same Receiver began again to leak and run over, so that I *set* the Screw again.

Nov. 27. Our Receiver seemed hitherto to be shut exactly enough, but this day I opened it, and, whilst the Air was getting out, the Peaches bubbled very much ; one of them, of the sort of those, to which the Stone, or Kernel useth to stick, had preserved its firmness, and afforded a taste pleasant enough ; but the other, being of that sort, which are of a yellow colour, was very soft, yet the taste thereof seemed to be more pleasant than the taste of the other. The liquor was very pleasant and grateful.

Decemb 28. As yet the other Receiver seemed unaltered ; but when I opened it, an innumerable company of Bubbles did immerge from the Liquor, and from the Peach. The Peach on one side had preserved its firmness, on the other it had lost it ; but the whole Peach was acceptable to the Palate, yet somewhat sharp.

This Experiment seems to teach us, that Liquors may grow fowre, though no Spirits have evaporated from them.

EXPERIMENT III.

September 20.

I included Peaches, with unripe Grapes, in two Receivers, and weighed them exactly. In the one were Apples bruised to the consistency of a Pultis : In the other, an Infusion of Raisins of the Sun.

Sept.

Sept. 25. The Receiver filled with pulp of Apples, hitherto seemed unaltered; but in the other, the Air which was generated, had extruded the half of the contained Liquor, and impelled the Mercury into the Gage, to the height of 100 digits; wherefore I opened the Receiver, and the Peach, whilst the Air was getting out, was almost reduced to the consistency of a Pultis; the taste of it was pleasant enough.

I put another Peach into the same Receiver, and substituted a new Infusion of Raisins of the Sun, instead of that which was lost.

Sept. 26. The Mercury was now come to 30 digits above its wonted height.

Sept. 27. The height of the Mercury was 72.

28. The height of it was 90. The Liquor did work out.

30. The same height remained, but the Liquor was all gone out.

October 1. I now perceived that all the Air had also escaped; Wherefore opening the Receiver, I found the Peaches very soft, yet of a pleasant taste.

Octob. 3. The Receiver filled with the pulp of Apples, had as yet lost nothing; but this day I perceived that almost all the juice of the Apples had run out, I opened the Receiver, and found all therein very much fermented. The Peach was very soft, but in taste not unpleasant.

This Experiment informs us, that Fruits cannot be long kept in pulp of Apples, by reason of the great production of Air; though that happens a little later in the Infusion of Raisins.

EXPERIMENT IV.

Sept. 23. 1678.

I included Peaches with crude Grapes in two Receivers, one of which was exactly filled with pulp of Apples, the other with unripe Grapes bruised.

Z

Octob.

Octob. 1. The Receiver filled with pulp of Apples, seemed as yet to have received no alteration; but the other was this day found emptied of his Wine: this therefore I opened, and found one of the Peaches to have retained its firmness, and its taste; but the other had lost its firmness, yet retained a grateful taste.

Feb. 5. 1679. The Receiver containing the Pulp of Apples, hitherto seemed unaltered; yet I opened it, and the great ebullition thereupon, did manifest, that a mighty compression of the Air was in it. The pulp of Apples and the Peach had kept a grateful taste, but somewhat more pungent than ordinary.

This Experiment shews us, that juice of crude Grapes cannot conveniently be used for the preservation of Fruits, by reason of the production of too much Air.

EXPERIMENT V.

Sept. 25. 1678.

I included two Pears, called Butter Pears, in a Receiver exactly filled with pulp of Apples.

Sept. 28. Hitherto I perceived no alteration in the height of the Mercury.

Octob. 5. The Mercury was now come to the height of 15 digits.

Octob. 6. The height of the Mercury was 16 digits and more.

Octob. 12. The Mercury was not changed.

Octob. 20. Three days ago the Mercury was depressed, though nothing had escaped out.

Octob. 26. This day my Receiver was found cracked, though I did not find that the Air was compressed within, but perhaps the Screw was set too high. The pulp of the Apples was of a very grateful taste; so were the Pears, but they were very soft, and one of them seemed to incline to rottenness.

Per-

Perhaps the crack in the Receiver was the cause why so little Air was produced in this Experiment.

EXPERIMENT VI.

Octob. 1. 1678.

Inclosed Peaches in two Receivers, one of which was filled with pulp of Apples, and the other with unripe Grapes bruised.

Octob. 5. Much Air was produced in the second Receiver, yet some of the Wine ran out. The height of the Mercury was 64 digits.

Octob. 6. The Wine proceeds to run out: the height of the Mercury was 70.

Octob. 8. Now the Wine was all run out of the Receiver, and the height of the Mercury was 86.

Octob. 12. The height of the Mercury abode at 86.

Octob. 18. That Receiver, out of which all the Wine was run, yet held the Air very well; and the height of the Mercury in it, staid at 86. The other Receiver, filled with pulp of Apples, had for these five last days suffered some juice to flow out.

Decemb. 4. I opened the Receiver filled with pulp of Apples, and though all the juice was got out, yet it still contained the Air, very much compressed; and many Bubbles brake forth, not without some noise, after the Receiver was quite opened. The Peach was very soft, and of a pungent taste, like to that of inebriating Wine.

Jan. 28. 1679. After the effusion of the Wine in the other Receiver, the Mercury staid in the same height. I opened the Receiver; the Peaches did emit many Bubbles, and were wrinkled, but their colour was little changed: their sapor was most pungent, and inclining to acid.

This Experiment doth confirm the Conclusions of the former.

E X P E R I M E N T VII.

Octob. 4. 1678.

I put Peaches into three Receivers; The first of which was filled with Ale, or Beer without Hops; the second with Beer Hopped; the third with Wine.

Octob. 5. The height of the Mercury in the first Receiver was 15 digits; in the second, 10; in the third 9 digits.

Octob. 6. The height of it in the first Receiver was 25 digits; in the second, 15; in the third, 20.

Octob. 8. The height of the Mercury in the first Receiver, was 35 digits; in the second, 15; in the third, 20.

Octob. 12. The height in the first Receiver was 63 digits; in the second, 15; in the third, 28.

15. The height of the Mercury in the first Receiver was 81 digits; in the second, 15; in the third, 30.

16. There was no more change perceived in any of the three Receivers.

18. The Mercury rather descended than ascended, in all the three Receivers.

22. In the Wine onely, the Mercury ascended or descended according to the heat and the cold.

24. The height of the Mercury in the first Receiver was 96 digits; in the second, 15; in the third, 30.

30. The height in the first Receiver was 115 digits; in the second, 20; in the third, 30.

Nov. 3. The height in the first Receiver was 117 digits; in the second, 20; in the third, 30.

6. The height in the first Receiver was 120 digits; in the second, 31; in the third, 31.

11. The height of the Mercury in the first Receiver was 105 digits; in the second 31; in the third, 28. It was cold weather.

Nov.

Nov. 16. The height of the Mercury was the same. The Peach, which hitherto was demersed, now mounted up to the upper part of the Liquor in the second Receiver; all the rest staid in the bottom.

Nov. 25. The height in the first Receiver, was 140 digits; in the second, 47; in the third, 32.

Nov. 28. The height in the first Receiver, was 96 digits; in the second, 36; in the third, 28. It was very cold weather.

Decemb. 13. The height in the first Receiver was 96 digits; in the second, 47; in the third, 33. I opened the third Receiver and found the Peach firm, and of a laudable colour, but it had contracted much of taste from the Wine, which yet was capable of being amended by Sugar, so that a very pleasant and edible dish might be made thereof. The Wine also was grateful to the palate.

Decemb. 30. The height of the Mercury in the first Receiver was 96 digits; in the second, 47. I opened the first Receiver, and the Peaches, which had lain till then at the bottom of the liquor, did presently emerge to the upper part thereof; they emitted many Bubbles: the taste of the Ale, of which they had contracted much, was made pleasant with Sugar.

This Experiment informs us, that fermented Liquors may be useful for the preservation of Fruits, as being unfit to produce Air.

EXPERIMENT VII

Sept. 5. 1678.

I included one Peach not cut, with another, cut into pieces, in a Receiver; into which I after poured old Wine, till it was exactly filled, and then shut it with a Screw. I hoped the issue would have been, that if the Wine did extract any tincture from the Peach, that the cut Peach would easily supply it; and so the whole Peach would keep its full taste.

Nov.

Nov. 20. As yet nothing seemed to be altered; but this day I perceived, that some of the Wine did run out.

Nov. 30. The third part of the Wine was lost.

Decemb. 8. Seeing the Wine begin again to run out, and that there was little of it left, I opened the Receiver, and found the Peaches very much fermented, yet endued with a grateful, but most pungent taste. The Wine also was pleasant.

By this Experiment, if it be compared with the third Receiver in the former Experiment, we may conjecture, that Wine doth hinder the fermentation of Peaches, if it be in a sufficient quantity; but here the Wine was not sufficient, because the pieces of that Peach which was cut, did fill the whole Receiver, so that no room was left for the Wine, but in the interstices.

EXPERIMENT IX.

Octob. 11. 1678.

I put two Peaches, one whole, the other cut in pieces, into a Receiver filled with hopped and fermented Beer.

Octob. 12. In one nights space the Mercury ascended 3 digits.

Octob. 15. The height of the Mercury was 15 digits.

16. The height of it was 15.

18. The height of it was 12. It was very cold.

20. The height of it remained at 12.

22. Now the Mercury ascended again. The Cold abated.

Nov. 2. The height of the Mercury was 20.

3. The Mercury descended a little. It was cold weather.

6. The height of the Mercury was 28. The weather grew hotter.

8. The height of it was 33.

Nov.

- Nov.* 11. The height of the Mercury was 40.
 12. The height remained at 40. Some of the Beer wrought out.
 16. The height of it was 46.
 19. The height of it was 43. But much of the Beer was lost.
 21. The Mercury ascended not, but the Beer proceeded to work out.
 23. When the Beer was almost all wrought out, I opened the Receiver, and found the Peaches very soft, yet of a grateful taste, though they had been kept 9 hours in the free Air, after the Receiver was opened.

N. These Fruits were never quite ripe.

From this Experiment, if it be compared with the second Receiver in *Exper.* VII. it may be inferred, that Beer doth hinder the Fermentation of Peaches, and the production of Air, if it be in a sufficient quantity: but here there was but a little Beer contained in the interstices, which was not able to hinder the fermentation of the Peaches.

EXPERIMENT X.

October 19. 1678.

I included raw Beef in 3 Receivers; the first of which was exactly filled with stale Beer, forcibly intruded, so that the Mercury exceeded its wonted height by 60 digits. The second was also exactly filled with stale Beer, but here there was no compression made. The third was filled partly with the Beef, and partly with Common Air.

Octob. 20. In the first Receiver the Mercury was depressed to the twentieth digit beyond its usual height, though nothing at all had escaped out. In the second also, it descended a little; but in the third, it ascended somewhat.

Octob. 26. In the first Receiver the Mercury did sometimes ascend,

ascend, and then descend very irregularly; in the second it began to ascend slowly two days ago; in the third it was not moved at all.

Octob. 27. One piece of the same Beef, which was left in the Air, began to have an ill smell; and also the Mercury in the third Receiver began to ascend. In the second it proceeded to ascend by little and little; but in the first it seemed rather to descend.

Nov. 3. The Mercury in the first Receiver ascended not; in the second, the height of it was 20 digits; in the third it was 10 digits.

Nov. 5. I opened all the Receivers, and the two first did not stink at all, yet they had contracted a Smell from the Beer. The Flesh boiled in the same Beer, was found very tender, but its taste was bitter, perhaps by reason of the too great quantity of the Beer. That Beef which was included with common Air, when the Receiver was opened, did presently affect the nostrils with a stinking smell; yet when it was taken out, and accurately smelt too, it scarce seemed to stink. I included the same Flesh in the same Receiver, to trie whether new Air being admitted, would promote corruption.

Nov. 6. The height of the Mercury was 3 digits.

11. The height of it was 9.

25. The height of it was 20 digits.

I opened the Receiver, I found the Flesh so stinking, that I was forced to throw it away.

From this Experiment it seems to follow, that Beer may be convenient for the preservation of Flesh, especially if it be intruded by force into the Receiver; but this compression is soon abated, because the Air compressed in the same Receiver, is apt to enter into and pervade the pores of the Beer by degrees.

EXPERIMENT XI.

November 12.

I included Beef, as hardly as I was able to do it, in 3 Receivers: Into the first of them I poured Water, mixed with one fortieth part of Salt, which filled up all the interstices which were left betwixt the parts of the Flesh: In the second, some salt Water was in like sort contained; but it was intruded by force, so that the Mercury in the Gage ascended to 15 digits above its wonted height: Into the third Receiver, I poured no Water, and therefore those few interstices which could not be possessed by the Flesh, were left for the Air.

Nov. 13. The Mercury descended in all the Receivers, especially in the second, which had admitted the compressed Liquor.

Nov. 18. The two Receivers, which were not compressed, did not repel the depressed Mercury upward: But as for that whose Mercury had been impelled to 15 digits, and afterwards had descended most of all, it now returned almost to its former height. A piece of the same Beef, being left in the Air, began to have a bad smell.

Nov. 23. In the three Receivers Air was produced a new; but this day in the second the Mercury descended 3 digits, the height of it was 20: in the other two 'twas about 16. I opened the first Receiver, and the Flesh was not corrupted at all.

Nov. 30. I took the Flesh out of the Receiver which was put in without Salt, it did not stink at all; but being boiled, was very tender and of a pleasant taste.

Decemb. 6. I opened the Receiver into which I had forcibly introduced salt Water. The Mercury exceeded its wonted height 25 digits. The smell of the Flesh did strongly affect the nostrils, yet it did not stink. The Flesh put *in vacuo* sent

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forth

forth many Bubbles, which ceased not, but a pretty while after, the Receiver in which it was included, was taken out of the Pneumatick Engine; yet the Mercury in one hours space, came to the height of 3 or 4 digits. Afterwards I immersed the same Receiver so exhausted, in hot Water, and the Liquor contained therein, did bubble very much, though the Water from which it borrowed all its heat, did not boil at all; but so great a quantity of Air was produced, or else had entered from without, that the Receiver was quickly full. Afterwards the Liquor contained therein, did not bubble or boil, though it were immersed in boiling Water. I took out the Flesh, and found it pleasant and tender, yet less *so* than I expected, perhaps because it was not yet boiled enough.

This Experiment teacheth us, that Water, as well as Beer, may conduce to the preservation of Flesh.

EXPERIMENT XII.

Nov. 29. 1678.

I inclosed Oysters in 4 Receivers; In the first the Oysters were without their shells; and filled the whole space as exactly as we could; in the second, the Oysters, not taken out of their shells, were included with common Air: in the third, the Oysters also were included in their shells, and the remaining space of the Receiver was exactly filled with salt Water. All these 3 Vessels were firmly closed with Screws. The fourth Receiver was exhausted of Air, and it contained 3 Oysters in their shells, and eight taken out of their shells. When the Air was pumped out of this Receiver, the Oysters which were taken out of their shells, did emit many Bubbles, and those very great ones; but the 3 others underwent no sensible mutation, save that one of them did gape.

Nov. 30. In the 3 Recipients which were stopped with
Screws,

Screws, the Air seemed to be consumed, rather than produced; but the Mercury *in vacuo* ascended a little.

Decemb. 4. Whilest the Weather was cold, the Mercury ascended not; but now when the Cold began to abate, the height of the Mercury in the first Receiver was 7 digits; in the second, none; in the third, 3; in the fourth, 3.

Decemb. 5. The height of the Mercury in the first Receiver was 20 digits; in the second, 1 digit; in the third, 3; in the fourth 5.

Decemb. 7. The height of the Mercury in the first Receiver was 30 digits; in the second, 1 digit; in the third, 3; in the fourth, 8. Other Oysters, left at the same time in the Air, had a bad smell.

Decemb. 9. In the first Receiver the height was 30; in the fourth, 11. The rest were not changed.

Decemb. 13. There was no change in the 3 first Receivers, but in the fourth the height was 14 digits.

Decemb. 20. In the first Receiver the height was 46 digits; in the fourth 24; the rest were not changed.

Decemb. 21. In the first Receiver the height was 52 digits. in the fourth, 25: in the rest no change.

Decemb. 22. The height of the Mercury in the first Receiver was 60; in the fourth, 27: no change in the rest.

Decemb. 27. In the fourth Receiver the height was 29. the rest were not changed.

Jan. 1. 1679 The Oysters in the third Receiver had tinged the Water with a black colour.

Jan. 25. The Mercury *in vacuo* seemed still to remain almost in the same height. But this day some Bubbles were formed in the Turpentine, by the internal Air, about the Commisfure of the Cover with the Receiver. Therefore I opened the Receiver, and found the Oysters very stinking; I likewise opened the other Receivers, and found the Oysters of a stinking smell, and turned to a kind of viscons Gelly.

This Experiment seems to inform us, that Fishes do produce less Air than Flesh; and yet, that they will be corrupted, though they are fortified against the Air.

EXPERIMENT XIII.

Nov. 29. 1678.

I exactly filled a Glass Vessel with fresh Butter, not at all salted, and then stopped it with a Screw. A mercurial Gage was included in the same Vessel.

Nov. 30. In the night, the cold being very sharp, the Butter was condensed, for the Mercury came nearer to the aperture of its Gage.

Decemb. 2. The Mercury came nearer and nearer to the aperture of its Gage, perhaps because the Cold did daily increase.

Decemb. 5. The Cold being abated, the Mercury returned almost to its former height; part of the same Butter, being left in the Air, began to have a very bad smell.

Decemb. 7. The Cold again returning, the Mercury did also again come to the top of its Gage. The Butter left in the Air, smelt worse than before, notwithstanding, as yet it was edible.

Decemb. 24. The Butter had produced no Air; being taken out of the Receiver, it was of a grateful taste, except onely a little of the superficies, which was contiguous to the Leather that was spread over the Cover.

From this Experiment it follows, that Butter may be kept a great while, if it be defended from the contact of the external Air.

EXPERIMENT XIV.

Nov. 30. 1678. I filled two Receivers with Whitings; and that

that no Air might be left in the vacant spaces, into the one I poured Wine; into the other, Oysters, with their juice, without their shells; so that both the Receivers were exactly filled. When I had afterwards closed their Covers with Screws, the Air in the mercurial Gages was compressed; but in 3 hours space the Mercury again returned to its former mark.

Decemb. 2. The Cold increasing, the Mercury came nearer to the aperture of its Gage in both Receivers.

Decemb. 4. The Cold ceasing, the Mercury ascended very much in that Receiver wherein the Oysters were, but in the other Receiver it was not moved.

Decemb. 5. In the Receiver containing the Oysters, the height of the Mercury was 20 digits; but in the other, it was not yet returned to its wonted height.

Decemb. 7. In the Receiver with Oysters, the height of the Mercury was 40 digits; in the other, it continued still below its wonted height.

Decemb. 9. The Mercury in both Receivers was changed little or nothing.

Decemb. 20. When the Mercury was changed no more, I opened the Receivers, and both of them were found to be very stinking: And this seemed new to me in this Experiment, that the Receiver in which the Wine was, had admitted of corruption without production of Air; for hitherto all Bodies, whilst they were corrupting, had produced Air.

EXPERIMENT XV.

Decemb. 3. 1678.

I put raw Beef into two large Receivers, with Pepper and Cloves; and that no Air might be left in the interstices, I poured in Beer upon them, and no long time after, I found the pressure of the Air in the Receivers to be abated, the Mercury in the Gages coming to the open ends.

Decemb.

Decemb. 8. The Mercury did not ascend in either of the Receivers. I opened the one, that I might boil the Flesh, it was endued with a sweet smell, contracted from the Cloves; and the Liquor contained in the same Receiver, before it was boiled, did smell like Hippocras.

Jan. 2. 1679. I opened the other Receiver, and found no Air produced therein; the Flesh was not at all corrupted, and when I boiled it *in vacuo*, I observed, that if a more intense fire were kindled, the Air, or some Spirits, did make an eruption through the stop-cock, which was fastned to the top of the Receiver. The Receiver, being cooled, all the night, the day after was found almost quite empty of Air. The Flesh was very tender, and well tasted, onely it was a little over-boiled, for it had been kept on the fire 6 full hours.

We have a confirmation by this Experiment, that Beer may be useful for the preservation of Flesh, especially if the bitter taste thereof be corrected by some Aromatics.

EXPERIMENT XVI.

Decemb. 4. 1678.

I included 2 Larks, with some Beef, in a Receiver, all whose spaces unpossessed by the Flesh, I filled with Ale; and at the same time I filled another Receiver with the same sort of Beef, adding Beer also, but no Larks were put in with it.

Decemb. 9. Some pieces cut off from the Larks, and exposed to the Air, began to smell ill; but those included in the Receiver, as yet had produced but little Air; for the Mercury was not yet come to 5 digits above its wonted height. In the other Receiver it was not moved.

Decemb. 19. In the Receiver, which contained the Larks, the Mercury ascended no higher; for the Cover being broken, suffered the Liquor to run out. Wherefore I opened the Receiver, and boiled both the Beef and the Larks, which were not at all corrupted,

corrupted, but they seemed very acceptable to the palate; yea the Beef had contracted a pleasant taste, partly from the Larks, and partly from the Beer.

Decemb. 23. I opened the other Receiver, and the boiled Flesh seemed pleasant, yet not so pleasant, as that which was endued with a Venison-like taste from the Larks.

This Experiment shews us, that even tender Birds may be preserved long by the help of Beer or Ale.

EXPERIMENT XVII.

December 14.

I included Apples in 4 Receivers; in the first was an whole Apple, and all the spaces were filled with powdered Sugar: in the second, an Apple was cut in pieces, and the spaces filled with Sugar, as before: in the third an Apple was also cut, but the rest of the Receiver was filled with Water, wherewith $\frac{1}{2}$ part of Sugar was mixed: in the fourth, the Apple was also cut, and the spaces were likewise filled with a solution of one part Sugar, and 5 parts of Water.

Decemb. 21. This day in the first Receiver the Mercury began a little to ascend, yet the Sugar did not melt: in the second Receiver all the Sugar was melted, and the pieces of Apple were shrivelled, also they produced much Air when they were first put into the Receiver: In the 2. other Receivers the Mercury began also to ascend; but in the third, the pieces of Apple were very much corrupted, for their *skin* or rine was taken off.

Decemb. 22. Air was produced in all the Receivers, but the quantities of the Air produced, did not bear the same proportion amongst themselves, as the quantities of the Sugar; for in the second Receiver much Air was produced, but in the fourth the Mercury ascended less than in the third; and besides, in the first some Air was generated.

Decemb. 27. In the three first Receivers the height of the Mer-

Mercury was 10 digits; but in the fourth 'twas onely 6 digits.

Decemb. 31. In the first and second Receivers the height of the Mercury was 13; in the third the height was 15; in the fourth it was onely 9 digits.

Jan. 2. 1679. In the first and second Receivers the height of the Mercury was almost 14; in the third, 17; in the fourth, 11.

Jan. 7. In the second Receiver the height of the Mercury was 16 digits; in the third, 36; in the fourth the height of it was 15: but in the first the Mercury had not ascended, and something had escaped out of the Receiver, and therefore I *eased* the Screw; that I might dispose of it the better; and then the Air made an escape.

Jan. 9. In the first Receiver the height was 6 digits; in the second, 16; in the third, 39; in the fourth, 15.

Jan. 17. In the first Receiver the height was 13; in the second, 19; in the third, 56; in the fourth, 17.

Jan. 30. In the third Receiver the height of the Mercury was 76 digits, and the Liquor brake out, and therefore I opened it, and found the Fruit to have lost much of its taste, but the Water had contracted it, and was pleasant enough to the palate. In the second Receiver the Mercury ascended no more. I opened this Receiver also, and found the Fruit much more pleasant in this than the other; yet much of its taste was imparted to the ambient Sugar, so that it was found changed into a very good Syrup.

Feb. 16. The height of the Mercury in the first Receiver was 22 digits; but in the fourth, 33. I opened it, and found the Fruit to have lost much of its taste, and that the ambient Water had got it, and was thereby turned into a pleasant drink.

Feb. 27. In the first Receiver the height of the Mercury was 30 digits.

March

March 15. In the first Receiver the height of the Mercury was not changed, but this day I found something to escape out of the Receiver, and therefore I opened it, and found the Apple of a laudable colour, but the Pulp was spongy, and had lost much of its taste.

This Experiment seems to teach us, that Sugar is not so fit for the preservation of Fruits, as Fermented Liquors. See *Exper. VII.*

EXPERIMENT XVIII.

December 23.

I filled a Glass Vessel with Milk, and then stopped it with a Screw; and into another Receiver I put a Lark with Milk, and stopped it close.

Decemb. 24. This Evening I perceived that the caseous part was severed from the butyrous, in the closed Receivers as well as in the Milk, which at the same time I had left exposed to the Air.

Decemb. 27. I found no Air produced in the Receiver which held the Lark; but in the other, the mercurial Gage was spoiled.

Decemb. 31. The Mercury ascended in that Receiver which contained the Lark; but the Milk that was left in the Air at the same time that I stopped the Receivers, did stink 3 days ago.

Jan. 1. 1679. In the Receiver, wherein the Lark was included, the height of the Mercury was 10 digits.

Jan. 2. The height of the Mercury was $14\frac{1}{2}$. The Milk stagnant below the butyrous part, appeared of a red colour.

Jan. 4. The height of the Mercury was 19. Some white se was concentered in the bottom of the Milk.

Jan. 9. The height of the Mercury was 29 digits.

Jan. 25. I opened both Receivers and found the Lark to affect

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fect the Nostrils with a strong, though no fœtid smell, yet it had been kept 32 days; when it was boiled it was of a pleasant taste. In the other Receiver, the caseous part of the Milk was subacid and grateful, but the butyrous part was not sower at all.

This Experiment informs us, that sometimes Milk may be used with good success for the preservation of Flesh.

EXPERIMENT XIX.

Decemb. 24. 1678.

I put a Lark into a small Receiver, and poured Butter upon it, melted with a slow fire, till all the spaces were exactly filled, then I closed the Cover with a Screw.

Decemb. 27. The Mercury approached nearer to the aperture of its Gage; but the Butter seemed to be altered, for the lowest part of it was more yellow, and the middle more white than it seemed before the inclusion thereof; the upper part was fluid.

Jan. 5. 1679. The Mercury returned by little and little, to its wonted height.

Jan. 9. The Mercury was somewhat higher.

Jan. 28. The Mercury was little changed: I opened the Receiver, and found that part of the Butter which was contiguous to the Leather spread over the Cover, to be white, and of a very unacceptable taste. The Butter which was more remote from the Leather, was yellow and something graveolent, yet it was edible. But the Lark being roasted, was grateful to the palate, though it had been kept 34 days.

This Experiment seems to inform us, that Butter melted and hot, is not so successfully used for the preservation of Flesh.

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EXPERIMENT XX.

Jan. 4. 1679.

I included boiled Flesh *in vacuo* in a Receiver stopped with a Screw, and filled the interstices exactly with Broth of the same Flesh, which seemed a little too salt. Whilest I *set* the Screw, all things in the Receiver suffered a compression, and the Mercury ascended to the height of 6 digits into the Gage; but shortly after it returned to its wonted height.

Jan. 28. The Air was more and more consumed, so that the Mercury now descended to 8 digits below its wonted height. I opened the Receiver, and found the Flesh very sweet and tender. The Broth also had a *subacid*, but a very *grateful* taste.

This Experiment informs us, that Flesh, after it is boiled, may be kept long without prejudice, which is a great conveniency in long Voyages at Sea, so that perhaps there will be no need of salted Flesh. For after the raw Flesh hath been kept so long in Vessels stopped with Screws, till Experience shews that there is no danger of its corruption; then it is to be taken out, and being perfectly boiled, is again to be included in the same Receivers: And so without doubt it may be kept for a long time without Salt. See *Exper. XII.*

EXPERIMENT XXI.

Jan. 30. 1679.

I put raw Flesh into 2 Receivers; to the first I added Pepper and Cloves; in the second I mixed nothing, for I was willing to know, whether these spices would promote the production of Air, or retard it.

Feb. 11. The height of the Mercury in the first Receiver was 3 digits; in the second the height of it was below $1\frac{1}{2}$.

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Feb.

Feb. 12. The height of the Mercury in the first Receiver was $4\frac{1}{2}$; in the second not above $1\frac{1}{2}$.

Feb. 13. In the first Receiver the height of the Mercury was 6 digits and more; in the second, it was 3 digits. I boiled the Flesh of the first Receiver, after the manner before described, and it was very pleasant and tender.

Feb. 14. The height of the Mercury in the second Receiver was 5 digits.

Feb. 19. The height of the Mercury in the second Receiver, was 8 digits.

Feb. 20. The height of the Mercury in the second Receiver was 11 digits. I boiled the Flesh and found it very tender, though it had staid over the Fire in *balneo mariæ*, onely for 3 quarters of an hour. I put some part of this Flesh, before it was boiled, into a Receiver, and filled all the spaces as exactly as I could with the same Flesh, to try how long the Flesh might be preserved when the Air was so excluded.

Feb. 28. The Mercury ascended very little.

March 20. The height of the Mercury was about 16 digits. I opened the Receiver, and the Flesh seemed of a pleasant taste, yet inclining to corruption.

EXPERIMENT XXII.

February 10.

I put raw Beef into 3 Receivers: In the first, the Beef was seasoned with Pepper and Cloves; in the second, it was encompassed with salt Water; in the third, I put neither Salt nor Spice.

Feb. 19. Four days ago the Mercury ascended in the third Receiver; in the first also it began to ascend; but in the second it was not moved at all.

Feb. 21. In the first Receiver the height of the Mercury was 4 di-

4 digits and $\frac{1}{2}$; in the third, 10 digits; but in the second, there was no ascent at all.

Feb. 25. The height of the Mercury in the first Receiver was 6 digits; in the third, 19 digits; in the second, half a digit.

Feb. 26. This night there was no ascension of the Mercury in all the Receivers. I opened the third Receiver, and the Flesh, after boiling, was found very good.

The former Experiment seems to teach us, that Spices do hinder the production of Air; but the present Experiment proves the contrary. Whence this contrariety should proceed, I know not; unless it be, because, perhaps, I had left a space large enough for the Air in these Receivers; but in the former Experiment I filled all as exactly as I could with Flesh.

March 9. The height of the Mercury in the first Receiver was 8 digits; in the second, none.

March 12. The height of the Mercury in the first Receiver was 12 digits; in the second, 1 digit.

April 3. The height of the Mercury in the first Receiver was 11 digits; but in the second, it exceeded not one digit. I opened the Receiver, and boiling the Flesh, after my accustomed manner, I found it very tender, and of an excellent taste.

The Corollary from this Experiment seems to be, that the saltness of Water, included with Flesh, doth hinder the production of Air; but because there was so small a quantity of Water, compared with the quantity of Flesh. I do rather incline to think that less Air was produced in the second Receiver, because it was more exactly filled. And indeed if fresh Water had been used instead of salt, the matter succeeds after the same sort; but the chief Art to Preserve Flesh without Salt consists herein, That all Air be excluded from it, and that there be a great compression in the Receiver.

All these Experiments about the preservation of Aliments,
what

what great use they may be of for the transporting of Fruits, Venison, or other Flesh from places far remote to great Cities, and for the affording better nourishment to Mariners, I leave to the Reader to judge.

ARTICLE XVIII.

*Experiments concerning Elixation and Distillation
in Vacuo.*

EXPERIMENT I.

Decemb. 12. 1678.

I Put 2 ounces and 6 drachms of Beef into an empty Receiver, which was able to hold 22 ounces of Water. Then I put it into boiling Water for 3 hours; which being done, I exposed it to the Air to be cooled for a whole night; afterwards, using my Pneumatick Engine, I perceived, that the Air formed in the Receiver, could scarce sustain 3 digits of Mercury; and so deducting from the Calculation, a man may easily find, that Flesh, whilst it is boiled, cannot form Air enough to make an entire pressure in a Receiver capable of holding a double weight of Water: that is, If you include one pound of Flesh in an emptied Receiver, able to hold 2 ounces of Water, it will not generate Air that can remove the Cover from the Receiver, unless heat do confer much to produce the effect; but I confess that our Flesh was not boiled enough.

*See the Description of a Vessel to Boil and Distil in Vacuo,
pag. 19.*

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EXPERIMENT II.

December 23.

I inclosed 3 ounces of raw Beef in a Receiver able to hold 32 ounces of Water ; and when it boiled, having been long on the Fire, the Cover was forced from its Receiver, and so suffered the vapours to pass out : but because it was presently shut again, the fire being removed, the Receiver soon lost its internal pressure, so that being set again to the fire, it was a long time before it could force away the Cover the second time. I tried this again and again ; yea, unless the Receiver had been exposed to a very strong fire, the Cover would never have been removed ; but if the fire be kindled enough, sweet exhalations continually pass out.

Decemb. 24. The Receiver having been cooled during the whole night, was this day, by the use of the Pneumatick Engine, almost wholly evacuated. Whence we seem to have a confirmation, that the divulsion of the Cover, is not made by that Air, which can keep the form of Air, but from the Steams exhaling from the Flesh, and subsiding again therein, if they be hindered from egress, which may easily be performed, if we use not too fierce a fire in the empty Receiver, and so the loss of those sweet smelling vapours may be easily avoided.

EXPERIMENT III.

Jan. 21. 1679.

I put Paste without Leaven into an exhausted Receiver ; and also I inclosed another part of the same Paste in another Receiver, full of Common Air. I inclosed these 2 Receivers *in balneo mariæ*, stopped with a Screw ; and when they had staid there for 3 hours, having been exposed to a moderate fire, I opened the Receivers : The Paste *in vacuo* I found reddish, as
far

far as the superficies; but the other had admitted Water; and the Paste was not boiled enough, and therefore I put both Receivers again in *balneo mariæ*, where they staid an whole night.

Jan. 22. This day in the morning, I found the *balneum mariæ* quite cold; and the Paste, when it was taken out, was boiled enough, but it was covered with no crust. That which was included in *vacuo*, was interspersed with many cavities, but it seemed too inspid; the other contained no cavities, but afforded a more pleasant taste. Both the Receivers were found almost wholly emptied of Air.

EXPERIMENT IV.

February 3. 1679.

I enclosed Paste kneaded with Leaven in *vacuo*, and as soon as it had filled its Receiver with factitious Air, I transmitted it into that Receiver, which I am accustomed to use to boil Flesh in *balneo mariæ*; but when the Paste was thus removed out of one Receiver into another; it pitched or sank very much; yet when it had remained for 3 hours in a fervid *balneo mariæ*, the Bread made of it was interspersed with many cavities, but it was covered with no crust.

Feb. 5. I iterated the same Experiment, but this time the Paste was included in *vacuo*, in the same Receiver, which was afterwards put in *balneo mariæ*, and therefore there was no need to remove the Paste, and to expose it to the Air. Hence it came to pass, that the Bread made thereof, was much lighter than the former.

EX.

EXPERIMENT V.

February 12.

I included Rosemary with Water in the Vessel described p. 19. and when the Air was pumped out, I put the Vessel in *balneo arenæ*, and there came forth a Water endued with a very sweet smell; yea and some drops of essential Oil, smelling very sweet also, and affected with no *Empyreuma*. But when I opened the Stop cock for to let in the Air, the noise did so soon cease, that I judged much Air was produced from the Rosemary.

Feb. 13. I put the same Rosemary into the same evacuated Vessel, and administred a more intense fire thereunto, yet I could extract no Oil, neither sweet nor stinking; and besides the Water was less fragrant than the former.

EXPERIMENT VI.

February 10. 1679.

I boiled 1 pound of Flesh in *vacuo*, in the Vessel described p. 19. which could contain almost 4 pound of Water: the upper part thereof, which was made of Glafs, did hold the mercurial Gage, by the help whereof, I perceived that the Mercury had not ascended to the height of 3 digits, though the Flesh had boiled for 3 hours and more. It was not boiled enough, and its taste was ungrateful; and moreover, the Liquor which was formed of the condensed Vapours, afforded also an unpleasant taste.

Feb. 11. I iterated the former Experiment, but this time I sprinkled the Flesh with Pepper and Cloves; the issue was, that the Mercury ascended to the height of 6 digits, though the Flesh was boiled no longer than the other; it seemed very grateful to the palate, and the Liquor formed from the Va-

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pours,

pours, afforded a most pungent taste of Pepper; but it had contracted nothing ungrateful from the Flesh, as was done in the former Experiment.

From these Experiments made about Elixation and Distillation *in vacuo*, the Corollary seems to be, that such Vessels may be very useful for the Distilling, and boiling of such bodies, which do contain thin, and very volatile Spirits: for all things will be preserved by their help, and nothing will avolute or flie away.

A R T I C L E X I X .

Concerning Elixation in Vessels stopped with Screws; by the help whereof, even Harts-horn, and the bones of Fishes, and Four-footed Creatures may be softened.

E X P E R I M E N T I .

• January 29.

Eight days ago I filled a Vessel, stopped with a Screw, with Beef and Water together, and when it had continued, exposed to a moderate Fire for eight or nine hours *in balneo mariæ*, stopped also with a Screw; I took the Flesh out of it, but it was boiled a great deal too much, and the Taste of it was very unpleasant. Afterwards, I boiled new Beef in the same Vessel, and after the same manner, save that this was seasoned with Pepper and Cloves, and remained exposed to the Fire, onely for three hours. The issue was, that this Flesh preserved a most pleasant taste; wherefore,
that

that I might know whether the excellency of this Flesh above the other, did proceed from the Spices, or from a shorter time of boiling, I boiled other Flesh without Spices for 3 hours, in the same Vessel, and after the same manner: when the Flesh was taken out, it was of a good taste. Whence I conjectured, that the cause of spoiling the first Flesh, was to be chiefly ascribed to the over-boiling: Yet I think that the Spices may be convenient to correct some part of the ungrateful taste; for I left a place for the condensing of the Vapours, in the top of the Vessel, and found that the Liquor there formed, was of an unpleasant taste; but when the Flesh was seasoned with Pepper and Cloves, no such thing was found.

EXPERIMENT II.

Jan. 29.

I boiled Apples, after the same manner as I did the Flesh before described; but I mixed no Water with them. They were set upon a moderate fire almost for 2 hours. They were very soft, and of a very good taste, but some pieces which were laid in the upper part of the Receiver, where the Vapours ascending from the inferiour part, were condensed, were found of an unpleasant taste; and also the drops, formed from the same Vapours, did affect the Nostrils with an ungrateful odour.

EXPERIMENT III.

February 4.

I enclosed Flesh with Pepper and Cloves in a Receiver, stopped with a Screw, but poured no Water in to fill the interstices, onely I compressed the Flesh, as much as I could, and then I put the Receiver in *balneo mariæ*, already hot, and stopped it with a Screw; and when it had remained there, over a moderate fire, for a whole hour, the Flesh was rather over-boiled than

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under-

under-boiled: But when I opened the *balneum mariæ*, all the Water brake out of it with a great force, *viz.* the Liquor being hot, and hitherto incarcerated, now having freedom given, at length did shew its strength.

Feb. 5. I enclosed some part of this Flesh in a Receiver stopped with a Screw.

March 12. The Flesh, which was included 5 weeks ago, was this day found very good. I do not doubt, but that perfect Elixation, was able to contribute something to its preservation, *viz.* because the sundry principles, of which Flesh consisteth, had, whilest the heat continued, exerted their strength upon one another, far better than if the Flesh, being less boiled, by reason of the great avolation of parts, had been to be removed from the Fire, as it happens in ordinary coctions. And indeed, by Experiments made about other Bodies, I have found that Elixation, the perfecter it is, doth so much the more hinder fermentation. See *Artic. XVII. Exper. XII, XX.*

E X P E R I M E N T I V.

February 10.

I boiled an Ox-foot or *Cow-heel*, after the same manner, as I had done the Flesh above mentioned, but I left the *Cow-heel* for 4 hours or more, upon a moderate fire. That time being elapsed, and the Vessels unstopped, the Flesh was excellently well boiled, and the bones were so soft, that they might be cut with a Knife, and eaten like Cheese.

Feb. 12. I repeated the same Experiment, but the Vessels remained exposed to the fire for 12 hourspace; and though the Water of the *balneum mariæ* did every where secure the Vessel demersed in it, yet the Flesh had contracted a taste and a smell very *Empyreumatical*; but the juice, which in the former Experiment did concrete into a very firm Gelly, in this latter, could not be congealed at all.

By

By these Experiments it appears, That many bones and hard tendons, which we daily cast away as unprofitable, by the help of *balneum mariæ*, stopped with a Screw, may be converted into good nourishment.

EXPERIMENT V.

February 10.

I boiled a Fish, after the same manner as was described above, in *balneo mariæ* stopped with a Screw, but I mixed no Water therewith. The Fish staid upon the fire two hours, onely; then the Vessel being cooled and opened, the Fish was found of a very good taste, and his bones were so soft that they yielded to the pressure of ones finger, and the head of it could be eaten like its flesh. The juice of it in a short time did concrete into a Gelly of an hard consistence.

This Experiment is very useful for the boiling of Fish which are full of bones.

EXPERIMENT VI.

February 15.

I put Harts horn into a Receiver which was to be stopped with a Screw, and filled the intervals with Water, I included the Receiver thus stopped, in *balneo mariæ*, stopped also with a Screw, and so exposed it for 4 hours to a moderate fire; when that time was passed and the Vessels opened, the Harts horn was as soft as Cheese; and the juice did soon concrete into a very firm Gelly.

Feb. 17. I repeated the same Experiment, but no Water was included with the Harts-horn, and the fire lasted 6 hours under the *balneum mariæ*; when this was done, the Harts-horn was found very soft, but a little juice had excreted out of it, and that did adhere to the external parts of the Harts-horn in the form of drops of Gelly.

The

The Excellency of this *Balneum mariæ* is confirmed by this Experiment: For seeing Harts-horn it self can be boiled by the help thereof, without the mixture of Water, there is no doubt but all fresh Water, which is wont to be spent in Ships to boil Flesh, may be preserved for other uses of the Mariners. Furthermore, If we add what we have tried about the preservation of raw Flesh, and after of that which is boiled. (See *Exper. III.*) Doubtless we may conceive great hope, that many inconveniences which are wont to prejudice Mariners, both by reason of the saltness of their meat, and the putrefaction of their Water, will be almost wholly remedied and prevented. Neither let any man object that so many Vessels, and so exactly stopped, are very difficult to be procured; for daily experience doth evince, that very many mechanical instruments, far more difficult, may in a little time become very easie for use, and as easily procurable.

FINIS.

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